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ART. XII.—*Extracts from a Memoir of Samuel George Morton, M.D., late President of the Academy of Natural Sciences of Philadelphia;* by CHARLES D. MEIGS, M.D.*

* * * * SAMUEL GEORGE MORTON was born at Philadelphia, on the 26th January, 1799. His father, Mr. George Morton, a native of Clonmel, in Ireland, was descended from a respectable stock in that city, and was trained in early life amidst a family, in which the gifts of education were highly prized and abundantly enjoyed. For the purpose of augmenting his fortune, he early emigrated to America, and devoted himself to mercantile affairs in Philadelphia.

Here he was united in marriage with Jane, a daughter of John and Margaret Cummings of this city. The issue of this marriage was nine children; of whom six died in their infancy, leaving him one daughter and two sons, James and an infant child, Samuel George Morton, of whose character we are to speak.

Mr. Morton, still engaged in commercial pursuits, which had not as yet yielded him the golden fruits of fortune, was suddenly cut off by illness on the 27th July, 1799, leaving the young child an orphan only six months old. The widow and her three children thus bereaved, and left with limited resources, withdrew from the city, retiring to a place a few miles from the city of New York, called West Farms, which was at the time a settle-

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ment under the chief direction of the people called Friends or Quakers.

Here Mrs. Morton experienced much friendly regard, and her children kind protection, from the benevolent people among whom she had chosen her lot. Although not originally a member of the Society of Friends, she had relations or connections among them, and was, after a time, led to enter the society as a member, and to wish that her children also should be received, which was done; so that young Samuel being formally taken within their protecting fold, his earliest years were passed in the training and discipline of Friends.

As soon as the orphan was suited by age, he entered the school at West Farms, and was there taught the rudiments of letters. A memorandum in his diary shows that the first twelve years of his life were passed under this discipline; one in which he learned those lessons of moderation and self-control that are best received in the tender plastic age, before the loud voices of the passions have risen above the whispers of reason.

The progress that he made in learning, if we may take his own statement, was not so great as could be desired. Nevertheless, he had already acquired a passionate fondness for books of history, which he devoured; and he discovered an early taste for poetical composition, and was greatly addicted to the writing of verses. It is reasonable, therefore, to suppose that while his advances in school learning may have been slow, his spirit was already up and striving to escape from the barriers with which immature age and other circumstances had hemmed it in.

Morton's love for historical studies, thus early awakened, never ceased in all his after life; and it formed, perhaps, a principal element in his subsequent destiny. His love of poetry, too, and versification, accompanied him to the latest period of life; and though unknown as one of the poets of America, he improved his natural talent in that way to the production of pieces, showing him to possess a lively perception of the beautiful and sublime in nature, and a quick sympathy with every gentle and noble sentiment.

In the year 1808, the elder brother, James Morton, was invited to visit his uncle, Mr. James Morton, who lived in affluent circumstances at Clonmel. This kind intention to protect and aid his deceased brother's son was defeated by the death of the lad, which occurred at Clonmel, about 1811. The younger and now only remaining son was yet diligently occupied at the Friends' School, and though we have reason to believe he displayed no little readiness in learning, he complains that what he "learned in seven years, might, with a proper tuition, have been mastered in two." So that he was never content with the earliest of his lessons. After this he passed some time at Friends' School at West Town, Chester county.

In the autumn of 1813 he was in his fifteenth year, and for the purpose of higher teaching was transferred to the school then held at Burlington, New Jersey, under the auspices of John Gummere, of the Society of Friends, a gentleman of enviable reputation as an instructor of youth and as a highly cultivated and conscientious man. Here he spent six months under Mr. Gummere's tuition, devoting himself principally to the study of the mathematical sciences. In after years he was wont to reflect with much satisfaction upon this portion of his life, as one very profitable for his instruction and improvement, and he expressed the opinion that it was the only school in which he "derived knowledge commensurate with the time and labor consumed in study," yet even here he had occasion to lament that he did not learn as much as was to be expected; because he was wanting in the first principles of education. This was a reflection that appears to have distressed him; yet to it is perhaps due the great pains he subsequently took to make amends for early deficiencies.

Although placed under Mr. Gummere's care, and loving his work, Morton did not even there acquire any strong bias or affection for mathematics. He still preferred history, in the reading of which he took extravagant delight. Mr. Gummere's school was the last one that he attended.

Thus we see that young Morton, early cast upon the world without good educational training, was enabled, through his own resolute will and an innate affection for learning, so far to master every difficulty as to rise above the hopes entertained of him, and succeed in his scheme of life better than thousands of his cotemporaries, to whom every possible advantage was extended.

Leaving the Burlington school, young Morton now came to Philadelphia in the summer of 1814, to be entered as an apprentice in a merchant's counting-house, with a view to be fitted for the management of general business.

From his first entrance upon the duties of the mercantile house, he was impressed with a seated conviction that such was not his vocation, and it was ever an irksome task to fulfill the duties of his station there. Yet while strictly observant of his place, he found occasions in the pauses of his work to gratify his love of books.

In the year 1817, he lost his mother, which was a grievous loss to him. He fervently loved her on account of the tender regard she merited by her gentle and affectionate deportment to her children.

In the autumn of the same year, he got possession of a copy of Dr. Rush's sixteen Introductory Lectures, which he read with such delight that he definitively resolved, after their perusal, to adopt the profession of their celebrated author, and he never afterwards had real occasion to repent his determination to take so important a step.

At that time the late Dr. Joseph Parrish, who was at the height of his reputation as a physician and teacher, was accustomed to receive into his office many young gentlemen, students of medicine, who were there instructed by him. Dr. Parrish had also several assistants—young physicians, who joined in the instructions given in the school, by way of familiar lectures and demonstrations. This was the medical school into which Morton entered as a student, in 1817; and it was fortunate for him to be placed under such excellent tuition—one that has sent forth many physicians who have acquired celebrity both in medicine and the collateral sciences.

While under Dr. Parrish's private tuition, Morton attended the courses of medical lectures at the University of Pennsylvania, where he learned Anatomy and Physiology, Therapeutics and Materia Medica, as well as the principles and practice of Physic and Surgery.

These educational pursuits served in a good measure to supply the deficiencies of his earlier training. Nor can it be denied that these departments of medical learning do coincide with very expanded and liberal views of nature; and that the study of them leads to an acquaintance with the laws and phenomena of both the inorganic and the organic.

It may, I hope, be allowed me to remark here, that these studies, if rightly pursued, might well serve to retrieve one's losses of time and errors of aim, through imperfect tuition in schools and colleges; and that it is not surprising that a youth, faithful to himself as Morton was, should have issued from such a medical school, far more advanced in useful knowledge, than he would have been by the devotion of an equal amount of labor and time in the ordinary academies or colleges of the United States.

Seeing how strong was his natural desire for mental improvement, one might well envy the happiness experienced by such an individual while imbibing the great and diversified streams of information so refreshing and strengthening to the spirit of an eager and thirsty aspirant. Such happiness is greatly to be desired as gratifying a commendable spirit and as laying up at the same time, treasures of knowledge, the dispensation and administration of which, for the benefit of others, afford a clear expectation of often renewed and calm delight during the lapse of a long and busy life.

I believe that in his studentship, Morton attended three courses of lectures at the University, and having at length complied with the rules of the institution in all respects, he was admitted to the degree of Doctor in Medicine at the Commencement, held in 1820. He was, in the same year, April 16th, 1820, elected a member of the Academy of Natural Sciences.

Though crowned with the honors of the University, he was not, as many young men are, puffed up with exaggerated notions of his abilities and accomplishments, for he was an unassuming, modest young man, who yearned for further and fuller knowledge not of his profession only, but of things and men—particularly of eminent physicians. He was also moved by a laudable desire to see and know those of his kinsfolk who still resided at Clonmel, the seat of his family and his ancestors, who equally desired to be acquainted with their young American relative, the only remaining son of a deceased beloved brother.

Accordingly, on the 10th of May, 1820, he embarked at New York, for the port of Liverpool, to visit them, agreeably to the warmest invitations from his uncle, Mr. James Morton of Clonmel, the same gentleman to whom, as his "Venerable Uncle," he afterwards gratefully dedicated the most considerable of all his works, the Crania Americana.

After a prosperous voyage he had the happiness to find himself surrounded by a numerous society of relatives and connections living in the most agreeable style, and possessed of manners, which could not but have a salutary influence upon him, and doubtless tended to impart to his own manners something peculiarly dignified, tempered by an extreme affability and urbanity, which ever afterwards distinguished him as a gentleman.

Morton did not, amidst the gay convivialities of his house, abandon himself to the pursuits of pleasure, but wisely devoted a portion of his time to studies that were occasionally broken in upon by journeys and by invitations to their hunts, their coursing the hare, their salmon-fishings, and their parties. He kept a regular diary of his proceedings, which shows that he was an attentive observer, not only of events and of nature, but of his own short-comings and his deficiencies in manners, in address, and his views of the world as it is. His diary shows him to have had, even at that time, a spirit earnestly engaged with itself in improving and strengthening its faculties, and rightly directing its aspirations.

Mr. James Morton, who soon became warmly attached to his nephew, was not satisfied that he should be content with the doctorate conferred by an American University, and insisted that he should possess himself of one which he regarded as more authoritative and distinguishing, namely, the Diploma of Edinburgh, at that day still boasting of some of its old celebrated names. Accordingly, it was determined that he should attend the curriculum at that school, with a view to obtain its honors.

Dr. Morton had reached Clonmel on the 14th of June, 1820. It was on the 20th of October that he arrived in the capital of Scotland to enter as a matriculate in the University.

Here he diligently attended the lectures of Professor Munro the younger, of Prof. Hope, Prof. Home, both the Hamiltons, and with great delight the geological lectures of Prof. Jameson. The last-named gentleman inspired him with a warmer zeal for natural science, though it is probable that his connection with Dr. Harlan, who had been one of Dr. Parrish's assistant instructors, had inducted him already into some positive acquaintance with and fondness for such studies.

Dr. Morton also attended the course of lectures delivered by the celebrated Prof. Gregory at the Edinburgh University, up to the period when those elegant discourses were terminated by the attack which resulted in the Professor's death on the 3d of April, 1821.

After a violent, dangerous, and protracted illness, which he suffered in the early part of 1821, and which brought some of his friends even from Clonmel to assist and console him, Morton made a tour in the Scottish highlands. The journal of this tour shows that, while he had a poetical sense of the beautiful and the sublime in Nature, he also cherished a habit of philosophical observation, and that he returned strengthened in body, and enlarged in his views by the visit to that interesting region. After a short delay at the capital he returned to Ireland to unite himself again with his family at Clonmel.

As Paris was then looked upon as a chief radiating point of medical science, it was determined that he should pursue his further medical studies there. Accordingly, he bade adieu to Clonmel again on the 4th day of October, and passing a few days in London and at other interesting points in England, he arrived at Paris on the 26th of the month.

Here he devoted himself very assiduously to his tasks as student, indulging very sparingly in the amusements and distractions of that seductive city. He was always on his guard against its temptations; and very frequently had before him the image of his deceased mother, as if warning and guarding him in the slippery paths of his youth; for he said that her spirit seemed to be always with him and about him; and that it was a sore trial of his young life, that by no longer adhering to the Society of Friends, he might grieve that gentle and loving spirit. He had tenderly loved his mother during her lifetime, and cherished her memory while he himself lived.

He told me that, when the carriage rolled out of the gates of Paris on his way to Italy, he looked back towards the metropolis, and upon the smoke and dust above it, and, raising his eyes and hands towards Heaven, ejaculated his thankfulness to be delivered from the moral contagion in which he had lived there, and thought of the cities of the plain.

No memoranda of his proceedings were made during his residence in Paris, but in his resumed journal he remarks that the past winter was the happiest of the three-and-twenty that had passed over his head, and that he had endeavored so to combine study with amusement as not to become weary of either.

He proceeded to Geneva and made an excursion along the wild romantic Glen of the Arve to the Vale of Chamounix. * * * *

Our traveller, pursuing his journey, visited Milan and Pavia, and thence proceeded to Turin. In fine, he spent the summer in visiting various places upon the continent.

The following winter was passed at Edinburgh again in sedulous attendance upon the lectures, and in active business at the Infirmary; so that, he certainly enjoyed remarkable opportunities to become acquainted with matters of his profession and make amends for the deficiencies of his early instruction.

At the commencement at Edinburgh held in August, 1823, he presented an elegant Latin Thesis, *de Corporis Dolore*, and having fulfilled the requirements of the Institution in his examination, was passed Doctor of Medicine of that school.

Certainly Dr. Morton must have now acquired solid claims to the character of Physician, in which vocation he was about to present himself in his native city; for he had commenced his studies in 1817, under a meritorious and able private teacher, and so had six long years of earnest studies and constant progress.

Dr. Morton ever looked with distrust upon his early education, which he regarded as incomplete, and therefore unsatisfactory; and hence he was always alive to the need of repairing the faults of it, if he would aspire to a dignified station in the great scholarship into which he was entered by the closing of his student-life. He applied himself therefore to obtain many accomplishments, so necessary for his purposes, in languages, in belles lettres, and what may be called matters of taste. He, however, still loved his history and poetry. His diary contains many selections and translations, both in prose and verse, from various Italian authors, whose language he read with facility.

He had acquired a good acquaintance with the Latin and French tongues, and some knowledge of the Greek. He never had leisure or pause from work to allow of his becoming master of the German language, which was always a subject of regret to him.

I cite these memorials of our departed friend and colleague the more willingly, inasmuch as they evince Dr. Morton's earnest appreciation of the advantages likely to enure to every scholar who will secure such facilities, while they also prove his good sense and prudence in early life; for he devoted himself to study, while others, loitering by the way, were happy to have escaped the thraldom of a pupilage or the trammels of a studentship, and

so fell into habits of ease and self-indulgence. The example is well fitted to be handed down for the imitation and encouragement of any youth, who would become a wise and accomplished gentleman and scholar.

It was in June, 1826, that Dr. Morton presented himself to the citizens of Philadelphia, as one of the physicians of the place. But the first appeals of the young physician fall upon ears that are deaf; and he determined that, while slow progress must be submitted to, time should not be lost. He renewed his connection with the Academy, which at that time embraced many names that need only to be pronounced, to show how ardent was the love of natural science here. Maclure, Ord, Lea, Say, Collins, Lesueur, Nuttal, the beloved and lamented Griffith, Harlan, Pickering, Conrad, and others, were inspired with a zeal that knew neither tiring nor satiety. The meetings of the Academy, which had before been held at the old hall in Arch street, were, in 1826, transferred to the new museum in Twelfth street, where Morton labored, with the others, in classifying and arranging the cabinet. Here he delivered an address, which contained an interesting history of the Academy from its foundation, and which has passed to a third edition.

His first scientific, published essay, was an article *on Cornine, a new alkaloid*, printed in the *Med. and Phys. Jour.* for 1825–6, p. 195.

May 1st, 1827, the young naturalist presented his first scientific communication to the Academy, which was entitled, *Analysis of Tabular Spar, from Bucks county, Penn., with a notice of various minerals found in the same locality.*

This paper, which was creditable to him, was followed by a long catalogue of articles, some of them very important, but too numerous and various to be cited here. They are enumerated at the end of this memoir.

The discovery of numerous marl-beds containing organic remains, in the State of New Jersey, and the extensive excavations of the Chesapeake and Delaware Canal, at that period in process of construction, furnished provocatives and aliment to his appetite for research; and his diligence enabled him to make numerous valuable contributions to the Academy's Journal, upon the subject of organic remains, and geological formations. More than fifty of them are noticed in its volumes. He also made contributions to the Am. Philos. Society, some of which are published in its Transactions; particularly his learned, ingenious, and admired essay, entitled *Crania Ægyptiaca*, which shows his ripe scholarship, and the fervency of his thought. This essay greatly enlarged his reputation abroad, and won the esteem of distinguished scholars.

To Professor Silliman's *American Journal of Science and Arts* he made valuable communications, and it is only necessary to turn over the volumes of his great correspondence, to perceive how largely the opinion of him was enhanced, especially by his celebrated essay on the *Cretaceous Group of the United States*, which contained a synopsis of the organic remains to be found in it; a work of much labor and research. His correspondence shows that this paper gained for him high credit, and drew forth happy compliments upon his ability, candor, and discrimination.

I shall beg your permission to remark that we have here a conclusive proof as to one point of Dr. Morton's character; I refer to what may be called his good sense; for it is in this country, and probably elsewhere, an evidence of good sense, when a medical man devotes himself with success, to the pursuit of Natural History, or any cognate study, without complete ruin to his prospects as a practitioner. * * *

He had early begun to make his now celebrated collection of crania, with great labor and toil, and inconvenient cost. He investigated organic remains; he explained problems in zoology and ethnology; he diligently attended the sick; he published valuable treatises on consumption, on the science of anatomy, and on the practice of physic. He served the city gratuitously, as physician to the Almshouse Hospital, and delivered courses of lectures at the Pennsylvania Medical College, where he was Professor of Anatomy. All these things were done by a man whose family was large, and chargeable upon his funds, derivable in chief from his exertions as a physician. Is it too much to say that a man who could do this, and acquire in the mean time, the reputation of being one of the most considerable physio-philosophers of the Western Continent, was both a wise and learned man? Yet nothing could make him either vain or arrogant.

Upon a late occasion, a gentleman returned from foreign travel, reported a compliment paid to him abroad in one of the very highest quarters in the whole republic of letters. Dr. Morton replied: "I beg you never to repeat that. I assure you it will be disagreeable to me ever to hear of it again." Yet certainly, if a man might accept a compliment, he could not desire a better or more disinterested one than that was which pronounced him to be the American Humboldt.

In 1834 Dr. Morton made a voyage to the West Indies, which gave him opportunity to investigate points in relation to their geological structure.

On the 18th September, 1839, he was elected Professor of Anatomy in Pennsylvania Medical College, which office he vacated November 6th, 1843.

I have mentioned his collection of crania. His earliest publication on ethnology was the splendid volume illustrated with beautiful lithographs, entitled *Crania Americana*, fol., Philadelphia, 1839. Here I shall feel myself authorized to detain you a few moments in relation to some points connected with that elaborate work, which will continue a lasting monument of his learning, energy, and ability.

It is a very remarkable circumstance that men of science, and particularly the cultivators of the natural sciences, had, to a certain extent, forgotten to attend to the records of man as creature merely, and member of the zoological series, whose form and other physical attributes it would be interesting to know, as they have existed during the long persistence of that imperishable unit, the Genus Man.

Elucidations of man's ethnological characteristics could be best drawn, not from the historian, the geographer, or philologer, but from the observation of his real physical attributes in various zones, climates, continents, and epochs. Such investigations might well serve to throw a brilliant light upon many dark questions both in history and chronology, as well as in morals and faith.

No unquestionable palæontological remains of the race have ever been discovered, and Scheuchzer's *Homo Diluvii testis*, M. De St. Merry's Martinique Galibis, and the remains from Santas, in Brazil, were alike rejected as no antediluvian records.

That illustrious physiologist, Prof. Blumenbach, was the first to point out this *lapsus* of regard on the part of the learned, and to take measures to repair the wants of science in relation thereto. He accordingly commenced his collection of human crania, and in the year 1790 published the first of his *Decades Craniorum*, which work was continued at intervals until the last of the Decades appeared, in 1828, having been in course of publication for a period of thirty-nine years.

These Decades contain the highly expressive outlines of sixty-five crania with faint linear shadings, representing the peculiar form and appearance of various races and families of mankind. They are highly expressive delineations; because they convey most explicit notions of the cranioscopical peculiarities of different races of men.

Blumenbach was surprised to find that, in the course of more than three centuries since the revival of letters, during which the industry and zeal of the learned had been exercised in making natural history collections, *ad luxuriosum fere usque excessum*, it was, as yet, almost impossible to meet, in the richest museums, with any specimens illustrative of the natural history of man; and further, he was astonished to learn that the subject had been neglected by the most classical and voluminous writers of past

times, and that even Conrad Gesner and John Ray had passed it by in silence.—*Dec. Cran.* I, 4.

Animated, therefore, with the idea of that important aid which might be given to the progress of knowledge by filling up this lacuna, he made his celebrated collection, and issued the publications before mentioned which were founded upon it. Much of his fame as a philosopher is derived from these labors. But, if the celebrated German has received meet praises for this work, shall we not also claim for an American physician and naturalist a share of applause for labors more assiduous, and results far more considerable and valuable?

The augmentation of his museum of crania of men and animals, made Dr. Morton, doubtless, to a great extent, aware of those cranial homologies so curiously set forth by the illustrious Oken, and which, if they may be clearly assumed as of the lower animals in various genera, must become in man merely differential ones; yet still, as Blumenbach and others, but most of all Dr. Morton have shown, easily appreciable. As ethnological attributes or marks, he certainly learned to discriminate and use them with great facility and exactness.

His valuable museum grew steadily up to the close of his life by frequent additions gathered from the whole world; and even since his death valuable specimens designed for it have arrived from foreign parts.

I desire not, gentlemen, to give undue praise to the subject of this memoir, and it is not from any such promptings that I beg to refer you to the eulogy pronounced by Dr. Pariset upon Cuvier, at the *Acad. Royale des Sciences* of Paris, July 9, 1833. Dr. Pariset wished to show how energetic was the spirit of Cuvier, in regard to the *Mus. d'Hist. Nat.*, at the *Jard. de Plantes;* and speaking of his appointment there, he says, "Il entre au muséum, et n'y rencontre que quelques squélettes incomplets ou vermoulous, qu'il faut tirer de la poussière. En quelques années plus de quatre cents squélettes de mammifères; plus de douze cents préparations osseuses; plus de seize cents organes d'animaux à sang-rouge et à sang-blanc conservés dans de l'esprit de vin, sortent des mains de Cuvier."

"C'est ainsi que, secondé de Rousseau et de Laurillard, ses fidèles ses zélés coopérateurs, il forme par dégrés chaque année, ou plutot par des rapides progrès, cette magnifique galerie d'Anatomie Comparée, la plus riche de l'Univers, où l'on compte aujourd'hui plus de deux mille cinq cents squélettes de mammifères, et d'oiseaux, de reptiles, de poissons; plus de quatre mille préparations osseuses; plus de six mille préparations d'organes; en tous plus de quinze mille pièces, dont plus de quatorze mille n'existaient pas avant lui."

Even the gentle and modest spirit of our deceased friend would take no alarm at this statement, which is not offered with a view of comparing him with the illustrious Frenchman, which is farthest from my thoughts. But I have cited the paragraphs merely to show how such great activity and zeal for science are, and ought to be estimated; yet I wished at the same time that some due estimate should be made of Dr. Morton's merits in a similar line of scientific effort. Dr. Morton began alone, and with nothing; without the patronage of government, or the assistance of imperial or royal treasuries, and with no Rousseau or Laurillard: yet, by means of his own pecuniary resources, never superabounding; overwhelmed with professional business; ofttimes in miserable health and in danger of death, he had, so far back as the year 1840, collected and arranged a cabinet of 867 human crania, from many widely separated regions of the earth, 253 crania of mammals; of birds 267, and of reptiles and fishes 81—making 1468 specimens, the number of which in the course of the last ten years has been very considerably increased.

I think that this is a just and yet modest exposition of his liberality, perseverance, and labor in behalf of science, leaving out of consideration the fatigue of so great a correspondence as was necessary to effect his object, and the inconvenient expenses, without which it could not have been carried on.

Dr. Morton never would have encountered all these toils for the gratification of mere curiosity, nor for any other purpose save that earnest one of getting at results, useful and applicable truths, to be turned to account in investigating the natural, moral or political history of man—objects in science surely not less interesting and exciting than the infusorials of Prof. Ehrenberg, or the metamorphoses of Sars or Steenstrup.

To compose and publish a great work in America is a bold undertaking on the part of an author. But, notwithstanding the demands of a growing family, which he loved with a love akin to idolatry, and for whom he desired to secure the priceless benefits of education, which he always deemed better than gold or much fine gold, or any endowment with worldly estate, he undertook, in 1839, to publish his great work, *Crania Americana*, which should contain the fruits of his labors and researches; and he also resolved that it should at the same time serve to illustrate an important department of the Arts in Philadelphia.

All this he did when he gave us his sumptuous volume, the *Crania Americana*, which issued from the press in 1839.

Notwithstanding this work did not, in his native city, produce the impression, and meet with the success which, from a liberal spirit among this community, might have been expected; the bare announcement of it brought numerous inquiries from abroad, showing the desire of many learned and eminent persons to know

to what results it might lead. In an appendix (p. 179, beyond) I have placed a letter from Baron Humboldt, which gives it the highest commendation.

Upon its publication it was welcomed, in the *American Journal of Science and Arts*, as "the most important, extensive, and valuable contribution to the Natural History of Man which has yet appeared on the American Continent.

"The subject," it was added, "is one of great interest, and Dr. Morton has treated it in a manner at once scientific and pleasing, while the beauty and accuracy of his lithographic plates are not surpassed by any of the modern illustrations of science." Such was the language of that most respectable Journal; and which allows me to say, *pulchrum est laudari à laudato viro.*

The *Medico-Chirurgical Review*, the *Western Journal*, the *British and Foreign Medical Review*, the *Journal of the Royal Geographical Society*, the *Eclectic Journal*, and that time-honored, able, and impartial work, the *Edinburg Medical and Surgical Journal*, all contain the most flattering encomiums upon Dr. Morton's labors.

I have no right to draw forth for the public from many private letters to him that I have had the pleasure to examine, the favorable sentiments and expressions of distinguished Naturalists and Archæologists, addressed to our deceased President upon the appearance of the *Crania Americana*. Could I with propriety do so, I might show you how great was the consideration it acquired for him in the highest literary and scientific quarters.

The work is on your table, and you are acquainted with its purposes and its contents as to our American Ethnology and Archæology.

You know that he considered "the Human species as consisting of twenty-two FAMILIES."

He did not assume that these FAMILIES are "identical with RACES, but merely groups of nations possessing, to a greater or less extent, similarity of physical and moral character and language."

He professes his belief in a primitive distribution of mankind into races, in the sense of their having been originally adapted to their local destination.

In classifying the races, he chose to adopt Blumenbach's methodical arrangement of those great divisions, *videlicet*, a Caucasian, a Mongolian, a Malay, an American, and an Ethiopian race.

Out of these divisions or races, he formed the seven families of the Caucasian race; as the Caucasian, Germanic, Celtic, Arabian, Lybian, Nilotic, and Indostanic families.

The Mongolians make five families; which are Mongol-Tartar, Turkish, Chinese, Indo-Chinese, and Polar families.

The Malays are two, the Malay family, and the Polynesian family.

The American race consists of the American family and the Toltecas.

The Ethiopian division comprises the Negro, Caffrarian, Hottentot, Oceanic-Negro, Australian, and Alforian families.

It would be no unpleasing task to exhibit here, in a concise manner, the exemplification, by many of his facts, of the justness of his views on these several topics; particularly as his pages are replete with interesting details and descriptions that render the volume a charming as well as a most instructive one. The style is grave, yet full of fervency; and the whole work is as modest and devoid of arrogance as Dr. Morton himself.

Humboldt compliments him by saying that his work is destitute of those poetical reveries which may be regarded as the myths of modern physiology.

It is far less loaded than the celebrated *Natural History of Man*, by Dr. Prichard; but in many respects it is equally deserving. We may well consider our country to have been honored by its publication.

I shall not add any further remarks upon it here beyond the expression of my hearty concurrence with the opinions of the *American Journal of Science and Arts*, as to the beauty of the lithographic heads, of the fidelity and truthfulness of which it needs only to be said that he was scrupulously honest to that degree as to condemn and cancel several completed lithographs, most perfectly executed save in some minor, yet, to his acute sense of truth, inadmissible want of perfect accuracy.

Want of time compels me now to turn to another of his highly interesting labors. I allude to the Crania Ægyptiaca, which, as I said before, was a communication made to the *American Philosophical Society*, in December, 1842, and January and April, 1843, and published as an independent work, entitled "*Crania Ægyptiaca, or Observations on Egyptian Ethnography, derived from History and the Monuments:*" Philadelphia and London, 1844, 4to., with plates.

This volume contains fourteen plates, with views of 98 heads, besides numerous very excellent wood-cuts inserted in the running text; and it may safely be said of the treatise, that though strictly a scientific production it is one highly pleasing even to the general reader, by the interesting nature of its topics, and by the relation, as the story proceeds, and the *perfervidum ingenium* with which it is pervaded.

His inquiries enabled him to come to the following conclusions:—

"1. The valley of the Nile, both in Egypt and in Arabia, was originally peopled by a branch of the Caucasian race.

2. These primeval people, since called Egyptians, were the Mizraimites of Scripture, the posterity of Ham, and directly associated with the Lybian family of nations.

3. In their physical character, the Egyptians were intermediate between the modern European and Semitic races.

4. The Austral-Egyptian or Meroite communities were an Indo-Arabian stock, engrafted on the primitive Lybian inhabitants.

5. Besides these exotic sources of population, the Egyptian race was at different periods modified by the influx of the Caucasian nations of Asia and Europe—Pelasgi or Hellenes, Scythians and Phœnicians.

6. Kings of Egypt appear to have been incidentally derived from each of the above nations.

7. The Copts, in part at least, are a mixture of the Caucasian and Negro, in extremely variable proportions.

8. Negroes were numerous in Egypt. Their social position in ancient times, was the same that it is now; that of servants or slaves.

9. The natural characteristics of all these families of man were distinctly figured on the monuments, and all of them, excepting the Scythians and Phœnicians, have been identified in the catacombs.

10. The present Fellahs are the lineal and least mixed descendants of the ancient Egyptians; and the latter are collaterally represented by the Tuaricks, Kabyles, Siwahs, and other remains of the Lybian family of nations.

11. The modern Nubians, with few exceptions, are not the descendants of the Monumental Ethiopians; but a variously mixed race of Arabians and Negroes.

12. Whatever may have been the size of the cartilaginous portion of the ear, the osseous structure conforms, in every instance to the usual relative position.

13. The teeth differ in nothing from those of other Caucasian nations.

14. The hair of the Egyptians resembles in texture that of the fairest Europeans of the present day.

15. The physical or organic characters which distinguish the several races of men are as old as the oldest records of our species."

Such are the inferences to which our president arrived after his long and arduous studies.

Dr. Morton, whose Museum I have so often alluded to, was naturally led by his ethnological studies to give a portion of his thoughts to what is called Egyptology, wherein he might haply derive materials for conclusion, very satisfactory to the mind, since the studies of the learned had lately acquired the highest importance as regards the fixing of many interesting chronological points.

The strata of the earth having hitherto disclosed no debris of antediluvian or palæontological man, if he could go back to the tombs and catacombs of the cotemporaries of Abraham and the Patriarchs, he might well hope ethnologically to verify specimens of the most ancient date, in his museum. Hence the foundation of this work, the *Crania Ægyptiaca.*

Fortunately he instituted a correspondence with a gentleman long an inhabitant of Egypt, and, by his varied erudition, and the most versatile talents and untiring zeal for learning, the fittest person in the world to aid him and promote his ends. I speak of Mr. George R. Gliddon, whose enthusiasm for Morton appears to have known no bounds; so that he was indefatigable in the search for and in forwarding to Dr. Morton, specimens of crania taken from various localities in Egypt, and so verified as to their chronological epochs and places as to give them the highest value as cabinet specimens. It is only necessary to refer to Dr. Morton's published works to see how effectual was the assistance he derived from that zealous and warm-hearted friend. But I cannot detain you longer with a relation of all that Mr. Gliddon effected and proposed in behalf of the subject of this memoir, nor can I sufficiently express my admiration of the heartiness and the spirit-like enthusiasm with which he helped and loved Morton.

They were both greatly moved by the disclosures of the Egyptologists, nor is it surprising they should have been. * * *

Dr. Morton, in his *Crania Ægyptiaca*, p. 39, asks the question, "Who were the ancient Egyptians?" and is inclined to admit the connection between the old name of Misraim the son of Ham, and Mizraim the old name of the Egyptians, as used by the Hebrew writers.

It matters not for the present, whether this idea be well founded or not; I having it in hand, only to show you where and in what manner Dr. Morton's thoughts were directed.

Perhaps Professor Rossellini may, with greater reasonableness, derive Misraim, or Mestraim, as Eusebius writes it, from *Tzur* a rock, or rocky-pass, whence *Matzur*, a fortress or castle. Rossellini says that Mitzraim is a word of dual signification, curiously indicating two rocks, to wit, the two rock-chains, Lybian and Arabian, that on either hand compress the valley of the Nile between their bases.—*Ross. Monumenti d'Egitto e Nubia*, i, 18.

Whether this question of Dr. Morton's can be best settled by the philologer or the archæologist, I shall not attempt to decide; yet I venture humbly to submit, that, in answering the query as to who were the Egyptians, it seems reasonable in him, to have thought that, if a word, or a name, or even a whole language can clear up the point, or if the stone stelæ, obelisks, temples, pyramids, and palaces can answer our demand, a plainer and more satisfactory solution is to be found, by bringing before us the very Egyptians themselves, as Dr. Morton appears, in some instances at least, to have successfully done. It is true that the skulls he describes, as from the pyramid-of-five-steps, he has not positively declared to be coeval with that most ancient structure; but if they are so, then they may, he thinks, be regarded as the

most ancient human remains at present known. Certainly they exhibit characteristics of the Caucasian race.

Of course it is not for me to entertain, much less express any opinion on the cotemporary age of Dr. Morton's specimens, and the new chamber discovered by Mr. Perring, in which they were found under circumstances leading to a suspicion of their extremest antiquity. Dr. Lepsius says of that pyramid land of Gizeh, Saqara, and Daschur, to which alone the Prussian commission devoted six whole months, "Wer sich aber, auch nur an die niedrigsten Annahmen der neueren Gelehrten über das Alter der ersten Ægyptischen Dynastieen halten wollte, würde noch immer jenen Denkmälen die Priorität vor allen übrigen Ægyptischen, so wie überhaupt vor allen geschichtlich nachweisbaren Kunstresten des ganzen menschlichen Geschlechts zugestehen müssen."—*Lepsius, Denkmäler aus Ægypten und Æthiopien*, p. 4.

But admitting that these skulls are as old as the foundation, you may start the question whether Dr. Morton could, in an immense collection of crania of all nations and of diverse epochs, unerringly select, and classify, and denominate those of the five divisions or races, and even families of mankind.

For my own part, I confess I long thought that his was a labor in vain; since to a casual observer as I was, the appearances noticeable in a great number of specimens are so similar, that I doubted the applicability of any rules of cranioscopical discrimination to the ends he had in view.

I now, however, fully admit that I did him injustice in the thought, which arose only from my own ignorance of the subject. I now believe that Dr. Morton's diagnostications as well as Blumenbach's, are to be depended upon; and that Dr. Morton's discrimination of the different skulls in his collection was so fine and delicate, that I cannot reject his indications and explanations of ethnological characteristics in them. I find an incident, mentioned in the diary, which I beg leave to relate to you, which is contained in a letter of his to his friend, George R. Gliddon, Esq., under the date of March 7th, 1845. The following extract from it, which I beg permission to read, gives proof of the ability with which he applied his cranioscopical learning to ethnological questions.

* * * * * * * * *

"yesterday sent me a copy of Bonomi and Birch's *Egyptian Antiquities of the British Museum*, which comes in good time, and is very welcome. Let me give you an example of the manner in which the study of physiognomy comes in aid of archæology. One of the first plates I examined was the third, fig. 36, which represents the Ægis of Athor, the Egyptian Venus. I

was at once struck with the resemblance of the profile to that of *Aamesnofri-ari* (*Cran. Ægypt.*, pl. 14, fig. 13), Queen of Amunophis the First; and on the principle that it was the custom of the Egyptians to make the features of the god resemble those of the monarch under whose reign they were executed, I was led to suppose that this effigy must in some way be connected with the Eighteenth Dynasty.

"Judge of my surprise and pleasure, upon reading the text, to find that this figure pertained to that very dynasty, though without any reference to the likeness between the effigies of the goddess and that of the queen."

If I am correct in these opinions, and if you coincide with them, certainly you will agree with me in deeming Dr. Morton warranted in bringing the light of his knowledge in these particulars, to bear upon the dim traces of man's lost history; and you will, upon due reflection, thank him even more than you have already done, and revere his memory and applaud his actions, who has built up that valuable collection now in your museum. That museum of yours is the scientific glory of the United States; and, it is fondly to be hoped, that the wealth and the luxury of Philadelphia, its fame for letters and philosophy, will not soon have occasion to be ashamed and penitent; as it must be, if that admirable collection, the fruit of so much toil and care, should be greedily and pitilessly taken from you, not by the greater wealth, but by the far greater liberality and public spirit of foreign nations or individuals. * * *

My duty now brings me to touch upon a subject of extreme delicacy; one which I should pass by in silence were it not that by doing so I should fall far short of a due obedience to your commands, which are to present to you a memoir of Dr. Morton.

I allude now to his published opinions on hybridity, involving notions upon the origin of the human race that brought him into conflict of opinion with others, whom he would be pained to disturb in their sentiments, and whom he could not expect to unsettle in matters of religious faith. Indeed he was peculiarly averse to such proceedings, and I happen to find in his diary, for Dec. 8th, 1849, an entry which shows his abhorrence of infidelity.

"Some self-sufficient Christians," says he, "endeavor to make other persons unhappy in their faith, by representing them as victims of delusion. Such attempts to destroy the sacred tranquillity of the human mind under its conscientious impressions of right and wrong are equally cruel and wicked with those of the true infidel, who would destroy our hope in the future by trying to convince us that there is no overruling Providence, and no existence beyond the grave."

Why did Dr. Morton make this entry into a private record of his thoughts? Certainly not with the expectation that I should see, or you should hear it.

In the *Crania Americana*, published in 1839, now twelve years ago, he had expressed his doubt as to the origin of mankind from a single pair, and he said, that "the prevalent belief is derived from the sacred writings, which, in the literal and obvious interpretation, teach us that all mankind must have originated from a single pair; whence it has been hastily and unnecessarily inferred, that the differences now observable in mankind are owing, solely, to vicissitudes of climate, locality, habits of life, and various collateral circumstances." And he asks whether man was not at once adapted by his Creator to the physical and moral circumstances in which he was to dwell upon the earth. He deems "that we are left to the reasonable conclusion that each race was adapted, from the beginning, to its peculiar local destination."

You perceive that Dr. Morton here asserts the physical characteristics of different races, as Europeans and Negroes, for instance, to be independent of external causes, and so aboriginal; or that the white man and the Malay, the North American Indian and the Hottentot, are people who could not have descended from the same original pair.

These views, which were forced upon him by an examination of the case, were not adopted without a reverent search of the sacred Scriptures.

The question before him was not whether all mankind are brethren, in the sense of being of the same species and under the same moral law, which as men they could not escape from being. It was a question relative to important facts in natural history and physiology.

He could not geologically admit that the Noachian deluge could at once cover the whole of the earth's surface; and he concluded that, if that great cataclysm which broke up the fountains of the deep, drowning vast extents of the earth, did leave some continents or parts of continents unsubmerged, the Scriptures would be rather strengthened and confirmed in their authority and dignity, than robbed and diminished in these respects, by a true reconciliation with the facts of geology, palæontology, and ethnology.

He thought the exceeding great populousness and the intellectual power and progress of the nations that existed at the founding of the pyramids, could be more reasonably accounted for by supposing a plural origin of pairs of the same moral stamp and responsibility, power and destiny, than by the natural increase and dispersion possible in so short a time as elapsed between the submergence of the deluge and the founding of those vast structures at Gizeh, Abusir, and Daschur—edifices which, whether laid by the hands of Menes or Cheops, go back to a remote date, now chronologically determined, beyond the days of Abraham.

These reflections led him to draw up a paper which was printed in the *American Journal of Science and Arts*, vol. ii, 2d Series, 1846, and entitled, "Some Observations on the Ethnography and Archæology of the American Aborigines." This was followed by an "Essay on Hybridity in Animals and Plants, considered in reference to the question of the Unity of the Human Species;" read at your table, but published in the *Am. Journ. of Science and Arts*, vol. iii, 2d Series, 1847.

The facts recited in these essays authorized him, as he conceived, to hold the following conclusions, which were summed up in these words, to wit:—

1st. A latent power of hybridity exists in many animals in the wild state; in which state also hybrids are produced.

2d. Hybridity occurs not only among different species, but among different genera, and the cross-breeds have been prolific in both cases.

3d. Domestication does not cause this quality, but only evolves it.

4th. The capacity for fertile hybridity, *cæteris paribus*, exists in animals in proportion to their aptitude for domesticity and cultivation.

5th. Since various different species are capable of producing together prolific hybrid offspring, hybridity ceases to be a test of specific affiliation.

6th. Consequently, the mere fact that the several races of mankind produce with each other a more or less prolific progeny, constitutes in itself no proof of the unity of the human species.

You see here what pains were taken by him to learn the facts in relation to hybridity in general, and to bring those facts to the decision of the question, as applied to the races and families of mankind! What a careful and pains-taking man he was; how conscientious as to the sacredness of truth, and how pure minded in the love of it!

Still, I am pained to say, these conclusions were attacked and controverted with such asperity, in different quarters, as to grieve him on account of the misconceptions that arose as to his inferences, and to prompt him to further inquiries and publications with which you are acquainted.

It seems to me probable that these attacks upon him would have been avoided but for the absence in science of any clear and unequivocal idea or competent definition, of the word *species*, with power to apply the term unerringly.

Dr. Morton says, at page 4 of the Essay on Hybridity, that, "where races can be proved to possess certain primordial distinctions which have been transmitted unbroken, they should be regarded as true species." But the impossibility of applying this test to the whole immense catalogue must make it often unavail-

able for the objects whether of classification or other philosophical deduction.

It may be said that there is nothing in nature so unchangeable as a species, which by a distinguished philosopher, M. Flourens, has been pronounced to be *un être collectif*, a collective being; *l'unité de la nature, impérissable en totalité*, the unity of nature, imperishable as to its totality; except in those cases where some overwhelming cataclysm comes to engulf a whole species and overturn the whole immutable order of the universe.—*Flourens, Cours sur la générat., l'ovologie, et l'embryologie.*

I should gladly present to you here the views of illustrious persons upon the nature and powers of species, did time allow. As it is, I may only say that Morton has himself given a definition of species, which is fit to be passed throughout the republic of letters as his own, and which has been beautifully complimented in a very high quarter. That definition of species is in these words,

"Species, a primordial organic form."

There is contained, among your archives, the letter to him from one of the most illustrious of living naturalists, which contains the beautiful compliment to which I allude.

Dr. Morton having made use of the so-called test of hybridity for the enlightenment of his own judgment, could not believe it applicable to settle the questions arising out of the ethnological differences of our race, and he merely announced that result—he had a good right to do so, if he spoke what he believed to be the truth upon a question in physics.

I beg you to understand that I am not called upon here to defend Dr. Morton, in his views of the great question whether the diversities, detected and figured by him, of craniological form, capacity, etc. etc., and which he insists are perennial, reaching down from the dates of the oldest human records and exuviæ, shortly subsequent to the subsidence of the flood, to the present time, be or be not sufficient grounds for his opinion. I leave these questions to the learned.

But may I not remark that the opposition to him and them probably arose more from some vague indeterminate apprehension, that they might be used as levers to disturb our faith in the Sacred Writings, than because they conflicted with inferences drawn by the highest authority in science, as Buffon, Cuvier, Flourens, Prichard and others, who, regarding species as unalterable, could not discern in the immense varieties of the human race, as to form, intelligence, color, and even endowment with organs, valid objections to the unity?

I trust I may further say I am quite sure the idea of disturbing any man's religious faith was foreign to his good heart, and, indeed, that neither his own religious tranquillity, nor that of his

opponents was ever in the least shaken by the doctrine of the geographical distribution of plants and animals.

That distribution, as a primordial constitution of them, whether examined as palæontological or as living species, is the inevitable conclusion of the mind; but the doctrine might, by some persons, be held as erroneous and offensive as that of the plural origin of man.

He conscientiously believed that the anthropology of the Scriptures ought not to, and does not, conflict with the notion of a plural origin of the race or races, which in nowise disclaims the unity of mankind as identical in species, and as brothers in moral and in physical nature, responsibility and destiny, power, hope, and free-will. * * * *

He did believe that he spoke the truth; and that truth, which is never irreconcilable with God's will and purpose, admits of being spoken and ought not to be hid. In this spirit, if this was his spirit, even though he erred, he was surely no conspirator.

I shall dismiss this part of my subject when I shall have presented to you a passage from that celebrated writer, the Chevalier Bunsen, showing *his* views upon a subject connected with the topic under review.

In his *Egypt's Place in the World's History*, the Chevalier Bunsen asks whether the study of Egyptian History would lead us to a conclusion that there was one universal deluge, or several partial and local floods; and whether the most ancient traditions, those of Egypt especially, exhibit any indications of violent interruptions in the early stages of human advancement; and lastly, what light is thrown by "our researches" on the great question of the unity of the human race and its primordial epochs.

"No historian," says he, "in these days, who deals honestly and conscientiously with Egyptian chronology, can evade these questions. We have no hesitation in asserting at once, and without entering into any further investigation, that there exist Egyptian monuments, the date of which can be accurately fixed, of a higher antiquity than those of any other nation known in history, viz., above 5000 years. The fact must be explained: to deny it would be a proof of little skill and still less candor on the part of any critic who has once undertaken to make the investigation."

Time bids me bring this memoir to a conclusion.

On the 3d of December, 1848, Dr. Morton was seized with pleuro-pneumonia of the left lung, which brought his life into imminent peril, at the time, but, after a severe and protracted struggle, left him to come forth again upon the stage of action a man in broken health from a ruined lung.

It was supposed he could never again engage in the pursuits of his profession; yet his indomitable courage and industry drove him into a still busier round of occupations, which increasing favor brought to him. After his illness he never breathed, save by the right lung alone.

Many of his medical friends exhorted him to spare a frame supported on such frail props of strength, by lessening his labor as physician; and they clearly indicated the premature dissolution that must follow any such course of exertions as his.

He was as well aware as the most sagacious of them of these risks; but he could not consent to live useless in the world, and so he labored diligently in his calling, and always, even in his illness, loved and remembered the Academy.

It was on Saturday the 10th of May, 1851, that, having spent the evening in the usual happy intercourse with his family, he was seized, towards the close of the evening, with a slight headache, which became violent on the following day, Sunday, the 11th, during which day he had also pain in the back and limbs. After having suffered severely from these symptoms, and from sleeplessness, he found himself on the morning of Wednesday, as he believed and said, free from disease, so that he slept tranquilly for several hours.

He now entered cheerfully into the affairs of his family, giving lively attention to the business of his household until, made happy by improving health, he sunk into a calm sleep.

He awoke in about an hour, and alarmed his friends by unmistakable evidences of great hebetude of the perceptive faculties, but expressed himself as free from pain. He was yet disposed to sleep, as soon as his attention ceased to be called. Very soon the power of deglutition was lost, hemiplegia of the right side was added to the mortal train, and he passed into profound coma, which terminated his existence at noon on Thursday, the 15th of May, 1851, in the 52d year of his age.

Dr. Morton was united in marriage to Rebecca, daughter of Robert and Elizabeth Pearsall, of this city, on the 23d Oct., 1827.

His home thenceforth was the secure abode of domestic peace, unity, and concord; and the happiness of that charming intellectual circle was broken only when disease or death could burst through the sacred spell of love and hope that bound, as in a protecting zone, its sweet repose.

It is proper to draw a veil over grief too sincere and too great to find consolation, save in religious confidence and hope. Nor should I take the occasion of this memoir even to make mention of a deep affliction he had in the loss of a noble youth, most hopeful and promising, who was cut off by death on the 15th May, 1850. Yet I venture to allude to it here that I may exhibit a specimen of his versification that shows, as well as many others

I have perused, not merely the intensity of his parental feeling, but also the graceful form in which he was ofttimes accustomed to express his sentiments in verse.

A Father's lament for his Son, George Morton, born December 21, 1832—Died May, 15, 1850.

"Stretched on the couch of anguish, lay
A youth, of manly form and graceful brow;
But lo! the strength of yesterday
Gives place to weakness now:
A day of agony—an hour of rest—
Then came the pulseless hand, and heaving breast,
And all was over. O! that sacred spell!
Wherein we prayed, and wept, and bade farewell!
That hurried warning of eternity!
That gush of wild emotions! O! my child!
Yet, thou alone wert calm and reconciled;
Death brought no fears to thee.
And art thou gone forever? Thou who seemed
An angel in my house and heart:
So young, so pure, so bright! I had not dreamed
That thus untimely we were doomed to part,
Or I should live to see the wild flowers bloom
Around thy early tomb.
Thy joyous step no more
Is heard by those who welcomed it before.
The sounding viol and the cheerful flute
By thee no longer touched, are hushed and mute;
And all is lone and sad where thou hast been;
Thy voice unheard—thyself unseen.
Yet, in our hearts thy memory shall live
Embalmed and beautiful, till life is o'er;
And then the promise of our faith shall give
Thy spirit back to us, to part no more
In that mysterious clime,
Where takes the soul no note of toil or time.

Thy tranquil grave is by the river's side,
And there our dust shall mingle with thy own;
And we will pray to die as thou hast died,
And go where thou art gone."

And alas! gentlemen, exactly one year after the loss of his beloved son, the good physician's grief was over, and the dust of the father and the child are mingled on the banks of the Schuylkill, at Laurel Hill.

His touching expressions reveal the truth of Morton's feelings; for he was a man of truth, and altogether above that sickly sentimentality that pours forth, in prose or in verse, expressions of passion never keenly felt, or grief never earnestly brought home to the life and the affections.

A volume of verses in manuscript lies before me, from which however, I shall not select any other specimens.

At his death, he left with his widow five sons and two daughters, who deplore the loss of a tender husband and parent, than whom I have not known one more excellent and amiable.

I have already said that his love for his family was almost idolatrous, and many of us who are witnesses of the graceful and unaffected hospitality of his house, can testify as to the marks of his love and confidence towards them: and we must, with one accord, regret the dissolution of those pleasant reunions, in which we have participated there, with men of letters and science of our own country or from foreign nations, who, with us, observed the cordiality and simplicity of his manners, in which were joined, in just proportions, dignity and urbanity.

Dr. Morton was a man above the ordinary stature; his face was oval, and always pale; his eyes a clear bluish-gray; his hair light.

As a man, he was modest in his demeanor, of no arrogant pretensions, and of a forgiving temper; charitable and respectful to others, yet never forgetful of self-respect. That he was a religious man I know from many opportunities had with him, and from his life and conversation. He was always in earnest and always to be depended upon.

Few men are to be found more free from faults, and few of greater probity or of more liberal sentiments, or of purer designs and aspirations. Doubtless he had faults, but they were not obvious, and I never discovered them in an acquaintance of near thirty years with him.

I have endeavored, while speaking of my friend, and while expressing my thoughts of what he was and what he performed, not to transcend the bounds of truth as to his character and his actions. I willingly give praise to all such Scholars; for I regard all men like him as fit teachers and guides for mankind.

Dr. Morton had the honor to be elected member of many societies of the learned in various parts of the United States, in Europe, and in the East, among which were the following:—

The Academy of Natural Sciences of Philadelphia. The Philadelphia Medical Society. The College of Physicians of Philadelphia. The American Philosophical Society. The United States Medical Association. The Massachusetts Medical Society. The Western Academy of Natural Sciences at St. Louis, Mo. The Georgia Historical Society. The Lyceum of Natural History of New York. The Boston Society of Natural History. The American Oriental Society at Boston. The American Ethnological Society at New York. The Medical Society of Sweden. The Royal Botanical Society of Ratisbon. The Academy of Science and Letters at Palermo. The Royal Society of Northern Antiquaries at Copenhagen. The Academy of Science, Letters, and Arts de Zelanti di Arci-reale. The Imperial Society of Naturalists of Moscow. The Medical Society of Edinburgh. The Senckenburg Natural History Society of Frankfort-on-Mayne.

The following are the titles of the published works of Dr. Morton, after which I have placed a translation of a letter addressed to him by the Baron Humboldt. [The list of his papers in different Journals, we propose to give in another place.—Eds.]

Synopsis of the Organic Remains of the Cretaceous Group of the United States, 88 pp. 8vo. With 19 plates. Philadelphia, 1834.

Illustrations of Pulmonary Consumption, its Anatomical Character, Causes, Symptoms and Treatment, with 12 colored plates: Philadelphia, 1834.

Mackintosh's Practice of Physic, with Notes by S. G. Morton.

Crania Americana; or, a Comparative view of the Skulls of various Aboriginal Nations of North and South America: to which is prefixed an Essay on the Varieties of the Human Species; illustrated by seventy-eight plates, and a colored map, by Samuel George Morton, M.D., &c. &c., folio: Philadelphia and London, 1839.

Inquiry into the Distinctive Characteristics of the Aboriginal race of America: 2d edit., 1844.

Crania Ægyptiaca; or, Observations on Egyptian Ethnography, derived from History and the Monuments; plates and wood-cuts: Philadelphia and London, 4to, 1845.

An Illustrated System of Human Anatomy, Special, General, and Microscopic: Philadelphia, 8vo, 1849.

Biographical Notice of the late George McClellan, M.D.: read before the Philadelphia College of Physicians, Sept. 4, 1849.

Translated copy of a Letter from Baron Alexander Humboldt to S. G. Morton, M.D.

Sir:—The close bonds of interest and affection that have for the past half century connected me with the hemisphere in which you reside, and of which I flatter myself that I am a citizen, have added to the impressions made upon me by the receipt, almost at the same moment, of your great work on Philosophical Physiology, and the admirable *History of the Conquest of Mexico* by Mr. Wm. Prescott. Works of this class, which extend by very different means the sphere of our knowledge, serve to add to the glory of one's country. I cannot sufficiently express my deep gratitude to you.

At my advanced age, I am peculiarly gratified by the interest still preserved for me beyond the great Atlantic valley over which a bridge has, as it were, been thrown by the power of steam.

The craniological treasures which you have been so fortunate as to unite in your collection, have in you found a worthy interpreter. Your work is equally remarkable for the profundity of its anatomical views, the numerical detail of the relations of organic conformation, the absence of those poetical reveries which are as the myths of modern physiology, and the generalizations with which your Introductory Essay abounds.

Being at present occupied in the preparation of the most important of my works, which will be published under the imprudent title of *Cosmos*, I shall know how to profit by so many excellent views upon the distribution of the races of mankind that are scattered throughout your beautiful volume. One cannot, indeed, but be surprised to see in it such evidences of artistic perfection, and that you could produce a work that is a fitting rival of whatever most beautiful has been produced either in France or in England. I pray you to accept the renewed expression of the high consideration with which I have the honor to be, sir, your obedient, humble servant,

ALEXANDER HUMBOLDT.

Berlin, 17th January, 1844.

Art. XIII.—*An Excursion on Etna;* by B. Silliman, Jr.

It was nine o'clock at night when we were summoned by our guide "Matteo" to mount our mules and follow his lead toward the great Cone of Mount Etna. We had prepared ourselves with suitable clothing for protection against the cold which we must encounter on the snow. My own dress was that of our American winter, besides which we were provided with warm woollen leggings of coarse yarn drawn over our boots and pantaloons above our knees. We had common gloves and over these thick woollen ones. Two shirts and a comforter for the neck, with an Italian Capote for the head completed our equipment. The ascent of the cone is seldom attempted so early in the season.

The sun had set in cloudless splendor, and as it rose on the morning of the same day, the summit of Etna was gilded with his earliest rays. Not a breath of wind was abroad. All the favoring circumstances gave us every reason to hope that our labor would not be in vain, and very naturally put us in good spirits for the wearying ride which was before us.

Besides "Matteo" we had also "Antonio," another experienced guide, judging it unsafe from the possibility of accidents to go up with one man only. Our party was therefore four men altogether, Mr. Brush and myself riding between the two guides. The night, as I have said, was serene, the stars shone in great brilliancy, and although there was no moon, we were soon able to see our way with sufficient clearness to inspire confidence. Our former experience with the mules had taught us that it was worse than useless to attempt to guide them, and that all we had to do was to sit still and let them follow the leader which they did with unerring step, seeming as if by instinct or by eyes in their shoes, to avoid every loose stone, and choose the securest foothold.

Our path lay for nearly or quite two hours over an unbroken waste of ancient lava, unwooded and with not a plant or vine to mark our course. This field we had not before traversed as our former excursions had taken us by other paths and away from this route leading towards the summit. On coming down in the morning we saw it of course with more distinctness and shall speak of it again. This tract bounds the fertile zone of vines and figs, although from the barren nature of the ancient lava at *this part of* the fertile district there is rather the appearance of a vast desert. Emerging from our stony path at the upper edge of the old lava, we suddenly entered the wooded zone, the commencement of which is as definite as the entrance to a cultivated park from a dusty road. This zone is one of the peculiar and most beautiful features of Etna, and demands especial notice by day. As we wound along our zigzag path slowly and cautiously, all we could discern was the shadowy form of huge trees widely planted, while the voice of a night songster told us of life and enjoyment in the vast solitude. We could tell also that the feet of our mules were treading soft sand, and the deeply worn path sometimes brought our feet in contact with the green sward. Mr. Brush and I had kept up a brisk conversation all the way, and the time past cheerfully and fast. We could hold no intercourse with our guides, who spoke only Italian; nevertheless we managed to make them understand nearly all that we had occasion to ask. About twelve o'clock we came upon a little hut where our guides dismounted and motioning to us to do the same, we found it was their purpose to feed the animals, which example we also followed by a resort to our provision

basket. Near our halting station was an immense tree under which we found an agreeable resting place—it measured about twelve feet in circumference. It was obvious from the glimpses we had obtained of the country below that we had risen to a great altitude, while the decreasing number of trees indicated our approach to the termination of the wooded zone. Before us the hills rose more rapidly, and we could also dimly discern occasionally a cinder cone. The air was sensibly cooler, and to our disappointment the wind had already risen to an uncomfortable breeze, which made it necessary to button up our coats and tie on our comforters. The mules being fed, we were again on our way, and in about twenty minutes, saw the last of the trees. An owl in a neighboring wood below us on the side of an ancient cone bade us farewell in a melancholy hoot, and we entered immediately on the desert zone. Our path at once became very rough and precipitous, now requiring us to grasp the mane of the mule, and the next to throw all our weight back to avoid sliding over his neck. But the patient, cautious creatures toiled on, pausing occasionally for an instant as if to reassure themselves, and then carefully advancing. Our guide too excited constant wonder. It was impossible to see a path—immense gulfs of rugged lava surrounded us; we found ourselves standing on the brink of precipices over which the course seemed to lead us, but a sudden turn carried us away just as the sense of danger was coming over us. There were to us no visible landmarks, and on every side in the dim distance of night we saw only an unending sameness of lava currents, ridges, gulfs, billows, and windings. Left to ourselves, we should certainly have given up in despair and waited the dawn; yet the guide was never for an instant at a a loss: not a word was spoken; our brisk conversation had died away in silence, and each seemed sufficiently occupied with the scene about us—for it was awfully sublime; and a thought of personal safety would perhaps occasionally present itself. Above us the snowy head of Etna floated like a cloud against the dark blue sky, and the constellations moved with our ascent, rising or falling as if with a more rapid revolution of the earth. The Great Bear was immediately before us, and every instant as we rose it sank until we soon hid from view the lower stars behind the cone of the volcano. It was now very cold, and we could distinctly trace at no great distance from us the snow in the deep gulleys of the mountain like white streamers from the great mass above. A few minutes brought us upon the lower patches, and from that instant the naked black rocks began to disappear, being replaced by the glistening snow. Our guide had several times shouted in a peculiar tone toward the west as if to arouse the echoes of the mountain. To our surprise his call was answered, and we were in some amazement to guess who should be in that lonely spot at

such an hour to return his salutation. The enigma was soon explained by the appearance of another guide to take charge of the mules. We had now reached the limits of our riding—we had been five hours in the saddle, and were now on the present confines of the snow. We dismounted, refreshed ourselves with some hard eggs and wine, took our mountain staffs and followed the guides who struck out immediately upon the snow. The wind now blew fiercely from the N. W., an ominous cloud was in the east, a heavy haze hung over the island, and I told Mr. Brush of my fears that we had no bright sunrise awaiting us. Still large portions of the sky were clear, and we had good courage to go on. I pointed Matteo to the cloud, when I found he had my fears; for he shook his head and said despondingly, looking to the cone, "molto vento." The ascent on the snow for the first mile or two was at an easy angle. The snow was crystallized like ice freshly broken, and soft enough to give us a firm foot-hold.

It was about quarter past two when we made our first halt at the pillar of stone erected at the base of the minor peak of Etna, which is called "Montagnuola." While stopping here over our basket of provisions, we observed in a striking manner the deceptive nature of distance, where objects are viewed from a great height and especially at night. We saw two lights, one of which we supposed to be the man with the mules (who had no light!) and the other we did not so clearly make out. On enquiry of Matteo, he told us that one (the first) was a light-house on the coast at Catania—twenty-five miles off—the other, the signal at Bronte, an equal distance on the opposite side of the mountain. Never was I so deceived by a physical phenomenon; I could have answered with the greatest confidence that both lights were within hailing distance. We now turned our course more northerly, the angle of ascent increased, and our exertions were arduous. We saw, as we thought, the ridge of the mountain just before us however, and over it the naked cone of Etna rising like an immense dome from the snowy waste. We pushed on to gain the ridge, and as it faded away another more distant presented itself. We looked back on the path we had come and forward, in hopes that the comparison would encourage us by showing that we had passed over the longer distance. No such comfortable assurance however was ours, and we would throw ourselves flat on our backs on the snow to regain freedom of respiration, and then push on anew. The wind was now fiercely keen, and so powerful, that had it been in our faces, I am persuaded we could not have made the ascent. Fortunately it was on one side, but it brought up frightful banks of clouds, while the wind cloud in the east had grown into massive banks of a dull grey, which hung directly over, where the first light of the morning indicated the position of the rising sun.

Our prospects were bad, and worst of all the apex of the cone was now invisible, while heavy masses of white vapor were constantly precipitated on it from the fresh portions of air which the wind brought in contact with the whole mountain side. We gained the "Casa Inglese," English house, just before four o'clock and were glad to find a shelter from the fierce wind under its gable—the only portion which was not buried in the snow. There was the cone immediately before us and at our feet—should we go up? pride said "yes"—discretion and the guide said "no." If the wind at the base would hardly allow us to keep our feet, what would it be on the unprotected summit 1300 feet higher? We concluded to wait for if we left our present position and gained the clouds we should not see the sun rise at all, nor any thing else. The whole horizon over Catania and Calabria was so hung with mist that we could see nothing distinctly. So we had no alternative but to wait where we were, in the hope that the rising sun would dispel the clouds and vapors and then ascend. At $4^h\ 25^m$ the sun appeared. It was a glorious sight. The dull clouds over the horizon were of a lovely purple and gold while a faint rosy light tinged the wastes of snow about us with an illusive warmth. But the glory was transient, the envious clouds shrouded his too brilliant glories, while the mists over Etna seemed every instant to thicken. We tried the temperature of the air and snow and found it to be $-7°$ C. $=20°$ F. At Catania on Thursday it had been 94° We had in fact, by an ascent of 9500 feet, made a difference of near 70° of temperature in a few hours.

There was therefore no alternative, we must abandon the idea of ascending the cone—it was useless and a mere waste of time and strength. Our guide, whose experience was great, decidedly opposed it and so we turned to see the *Val del Bove* from above. We had explored this vast gulf from below and were desirous of seeing what proportion it bore to the general surface of Etna. This we had hoped to do from above on the upper cone—the next best thing was to see it from the verge of the bounding precipice. Our path lay before the wind which was so violent that it was easy to keep up a full run and the loose snow kicked up by our feet was driven before us quite like a snow storm. It was near a mile to the edge of the precipice. I had been advised by Dr. Gemmellaro to keep my eyes shut until the guide placed me in a position to see the gulf and then to look in suddenly—as the best way to obtain a vivid impression. This I did: on raising my eyes, a scene of awful grandeur was before me, and the story it told was as plain in the history of past changes in the mountain as in a written record of human action. From this point the Val del Bove was somewhat quadrangular in form and even more grand in its ruins than when seen on its

bare plain. Here was a yawning chasm so deep and so wide that Vesuvius might be set down in it and have room to spare on either side, while its summit cone would hardly reach up to our feet! Vast indeed was the engulfment which had swallowed up so wide an area—and yet how small was that area compared with the whole surface on the flanks of this grand dome. In this view the remark of Spallanzani seems just, that compared to Etna, Vesuvius was a cabinet volcano.

The position of the sun, as it shone in our eyes, was not favorable for a good view of the valley; yet under the disadvantages, its bold crags, sharp wall-like dykes projecting from the sides, with a frigid yet tumultuous ocean of lava currents below us, were remarkably grand and impressive. "Capra" and "Musara" stand like sole remaining sentinels of the conflict in the midst of the scene; and in the distance lay the Mediterranean with black rivers of lava leading the eye to it:—all these and many other minor elements of grandeur combine to render it one of the most impressive and powerfully instructive scenes which it is possible to behold.

From the Val del Bove we turned our steps down the mountain in the general direction in which we had ascended. And now in the bright light of day, the weary wastes of unbroken snow seemed more extensive than on our ascent. We had been nearly or quite six miles over the snow but it was in the dark and we could not see its extent. Now on every side, we saw only an arctic winter, while below us, and almost in sight, were standing fields with vines, figs and olives, lemons, besides grain all yellow to the harvest. It was about 6½ when we at last reached our mules and took our *breakfast.* The descent we found more arduous than the ascent, not only because we were fatigued, but also on account of the great strain on the knees in holding back against the stirrup. As we went down the contrast of temperature again struck us very much. Our Etna clothing became oppressive, and one by one we cast off all our superfluous garments. On entering the wooded region we were enchanted with its beauty, although, as Dr. Gemmellaro assured us, it has been greatly injured of late years by the cutting away of some of the best trees. The wood is mostly oak with a few pines and firs; the trees are very large, not high, and at such distances from each other that they seem to have been planted by art as in a park. But the most beautiful feature of this region are the ancient cones whose slopes are grassed and also wooded. They retain perfectly their form, inside and out; the trees upon them are not thick enough to hide their forms while they give life and beauty in a surprising manner. These trees were completely vocal with birds as we rode through, but we recognized no familiar faces among the songsters. This morning we were too soon through

the beautiful zone of the trees, especially as succeeding them was a very long and tedious ride along the descent of the old lavas before named, before we could reach the village of Nicolosi.

It was excessively hot, mules and men were all thoroughly fagged, and I noticed as we rode along that our guide slept quite continuously on his mule. For myself I had not the least feeling of sleepiness. It was 9¾ A. M. before we reached the hotel, quite to the surprise of all, who were not looking for us until eleven o'clock. They were all amazed to hear of the wind we had experienced on the mountain, as it had been perfectly calm below, and they had thought we were entirely successful in our ascent. Yet unsuccessful as we certainly were in not reaching the summit, we had enough to repay us for the toil and exposure. The spirit of adventure alone is sufficient to satisfy most people who have confined their mountain rambles to the day. The snow also in its extent and massiveness entirely exceeded any expectations I had formed of it; and, above all, the Val del Bove, had we seen nothing else, would have rewarded us for the labor we had undergone. It is not merely for a sight of the rising sun that it is worth while to ascend in the night. The distance is such that the time consumed is necessarily from twelve to fifteen hours, and to do all this under a bright sun is a great exposure of health. It is far safer to descend from the climate of mid-winter to summer, than heated and moist with perspiration to plunge from summer into winter. The effect of a full sun also on the snow as reflected in the eyes is too much for the unprotected vision with the brilliant splendor of an Italian atmosphere. I think it then a wise course to go up at night, and no one need fear being overcome with sleep; the excitement of the occasion is too great. In summer, that is in July or August, the English house is free from snow, and then adventurers may carry up their mattresses and sleep there. This reduces the labor very much, since when the snow is off, mules may go quite to the Casa Inglese; and then the adventurer may engage with comfort and strength in his explorations. Dr. Gemmellaro has a plan for reconstructing the English house in the form of a pyramid, and in such a manner that access may be had to it in even the winter by an opening near the summit. The distance from Nicolosi to the summit of the cone is estimated by the guides at twenty-four miles, but Dr. Gemmellaro thinks it only fifteen.

[a] Nicolosi, May 30th, 1851.

ART. XIV.—*On Coral Reefs and Islands;* by JAMES D. DANA. Part Sixth.

4. *Origin of the Channels within Barriers, and of the Atoll Form of Coral Islands.*

IN the review of causes modifying the forms of reefs, no reason was assigned for the most striking, we may say the most surprising, of all their features,—that they so frequently take a belt-like form, and enclose a wide lagoon; or in other cases, range along, at a distance of some miles, it may be, from the land they protect, with a deep sea separating them from the shores.

This peculiar character of the coral island was naturally the wonder of early voyagers, and the source of many speculations. The instinct of the polyp was made by some the subject of special admiration; for the "helpless animalcules" were supposed to have selected the very form best calculated to withstand the violence of the waves, and apparently with direct reference to the mighty forces which were to attack the rising battlements. They had thrown up a breastwork, as a shelter to an extensive working ground under its lee, "where their infant colonies might be safely sent forth."*

It has been a more popular theory that the coral structures were built upon the summits of volcanoes;—that the crater of the volcano corresponded to the lagoon, and the rim to the belt of land; that the entrance to the lagoon was over a break in the crater, a common result of an eruption. This view was apparently supported by the volcanic character of the high islands in the same seas.—But since a more satisfactory explanation has been offered by Mr. Darwin, numerous objections to this hypothesis have become apparent.

a. The volcanic cones must either have been subaerial and were afterwards sunk beneath the waters, or else they were submarine from the first. In the former case the crater would have been destroyed, with rare exceptions, during the subsidence; and in the latter there is reason to believe that a distinct crater would seldom, if ever, be formed.

b. The hypothesis, moreover, requires that the ocean's bed should have been thickly planted with craters—seventy in a single archipelago,—and they should have been of nearly the same elevation; for if more than twenty fathoms below the surface, corals could not grow upon them. But no records warrant the supposition that such a volcanic area ever existed. The volcanoes of the Andes differ from one to ten thousand feet in altitude, and scarcely two cones throughout the world are as nearly of the

* Flinders.

same height as here supposed. Mount Loa and Mount Kea, of Hawaii, present a remarkable instance of approximation, as they differ but two hundred feet: but the two sides of the crater of Mount Loa differ three hundred and fourteen feet in height. Mount Kea, though of volcanic character, has no large crater at top. Hualalai, the third mountain of Hawaii, is 4000 feet lower than Mount Loa. The volcanic summit of East Maui is 10,000 feet high, and is a fine example of a large crater; but the wall of the crater on one side is 700 feet lower than the highest point of the mountain; and the bottom of the crater is 2000 feet below the rim of the crater. Similar facts are presented by all volcanic regions.

c. It further requires that there should be craters at least fifty miles in diameter, and that twenty and thirty miles should be a common size. Facts give no support to such an assumption.

d. It supposes that the high islands of the Pacific, in the vicinity of the coral islands, abound in craters; while on the contrary there are none, as far as is known, in the Marquesas, Gambier, or Society Groups, the three which lie nearest to the Paumotus. Even this supposition fails, therefore, of giving plausibility to the crater hypothesis.

Thus at variance with facts, the theory has lost favor, and is no longer sustained even by those who were once its strongest advocates. The question still recurs with regard to the basement of coral islands, and the origin of their lagoon character. Shall we suppose, with some writers, that these islands were planted upon submarine banks, within one hundred and fifty feet of the surface of the sea? As has been said, there is no authority for the supposition. We nowhere find regions upon our continents with elevations so uniform in height; and submerged banks of this kind are of extremely rare occurrence. If such patches of submerged land existed, the lagoon structure would still be as inexplicable as ever; for the growing reefs of the Pacific show that corals may flourish alike over all parts of the bank, where not too deep. The zoophyte can by no means be said to prefer the declivity to the central plateau of the submarine bank: on the contrary, the part nearest the surface appears to abound in the largest species of corals.*

A study and comparison of the reefs of different kinds,—fringing, barrier, and atoll,—throughout the oceans, is the only philosophical mode of arriving at any conclusion on this subject. This course Mr. Darwin has happily and successfully pursued, and has arrived, as we have reason to believe, at the true theory of Coral Islands. It is satisfactory, because it is a simple generali-

* Lieutenant Nelson, R. N., suggested this hypothesis before the publication of Mr. Darwin's views. See Geol. Trans., vol. v, 12; and Darwin, op. cit., p. 94.

zation of facts. The explorations of the Expedition afford striking illustrations of his views, and elucidate some points which were still deemed obscure, establishing the theory on a firm basis of evidence, and exhibiting its complete correspondence with observation.

A. *Channels within barriers.*—We may turn again to the chart of the Feejee Group, and glance successively at the islands Goro, Angau, Nairai, Lakemba, Argo Reef, Exploring Isles, and Nanuku.* In Goro, the reef closely encircles the land upon whose submarine shores it was built up. In the island next mentioned, the reef has the same character, but is more distant from the shores, forming what has been termed a barrier reef; the name implying a difference in position, but none in mode of formation. In the last of the islands enumerated, the barrier reef includes a large sea, and the island it encloses is but a rocky peak within this sea.

Can we account for this diversity in the position of barrier reefs, and in their extent as compared with the enclosed land? There is evidently one way in which these features might have been produced. If, for example, such an island as Angau were very gradually to subside, from some subterranean cause, two results would take place:—the land would slowly disappear, while the coral reef, which is ever in constant increase, as has been explained, might retain itself at the surface, if the rapidity of subsidence was not beyond a certain rate. This subsidence might go on till the last mountain peak remained alone above the waters. Should we not then have a Nanuku? Suppose the subsidence not to have proceeded quite as far as this, it might leave only a single ridge and a few isolated summits peering above the waves. Would not its condition in this case be that of the Exploring Isles? On such a supposition, reefs of large size encircling a mere point of rock might be explained in every feature. The subsidence of Goro, on the same principle, would produce an Angau, or, carried further, a Nanuku.

It may here be remarked, that the fact that changes of level in the earth's surface have taken place over vast areas, is fully proved, and accounts of some of them which are now in progress, as that of Sweden, are to be found in any geological treatise.

But it admits of direct demonstration that such a subsidence has actually taken place. It has been stated that the depth of the reef at different distances from the shore it encircles may generally be estimated from the slope of the shore. On this principle it has been shown† that the thickness of the distant barrier reef cannot be less in some instances than a thousand feet; and

* This chart will be found in copies of the Narrative of the U. S. Exploring Expedition, by Capt. Wilkes, U. S. N.

† This Journal, [2], vol. xi, p. 371.

in many cases it is probably much greater. Now as reef corals do not grow below twenty fathoms, there is no way in which this thousand feet of reef could have been formed except by a gradual subsiding of the land upon which it stands. The large number of instances of distant barriers in the Pacific remove any doubt with regard to these conclusions. The map of the Feejees abounds in them through its eastern part, and we may infer with reason that this has been a large area of subsidence, like that which is now going on in Greenland.

Evidence of subsidence still more conclusive, if possible, is obtained by actual observation at Metia and some of the elevated coral islands. This island is 250 feet in height, full twice the coral-growing depth. At another island in the Hervey Group, Mangaia, the coral rock is raised 300 feet out of water.

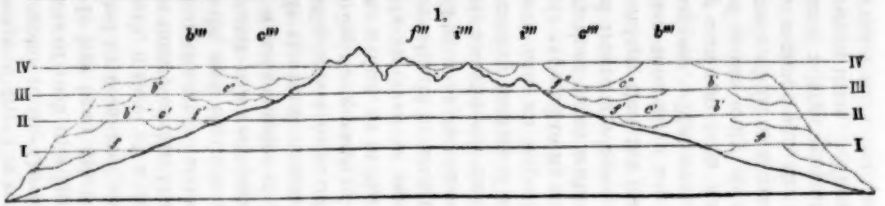
The fact of subsidence having actually taken place during the formation of many reefs, is therefore put beyond doubt. It must form a part of any true theory of reefs, whether it be the crater hypothesis or the view here advocated. The latter has this advantage, that it explains all the facts, and requires no other element but this single one of subsidence. It rests on a simple fact and demands no hypothesis whatever.

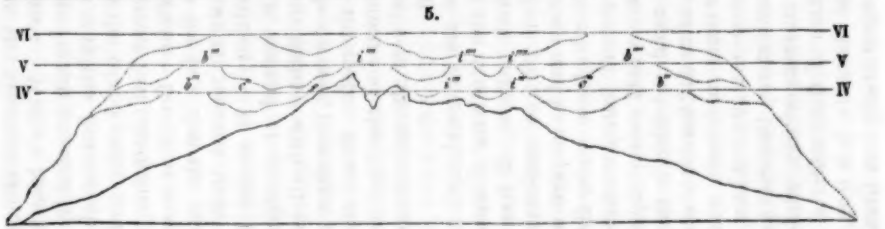
The manner in which subsidence would operate is shown in the following sketches, representing ideal transverse sections of an island and its reefs. In figure 1, if I be the water line, the island, like Goro, has a simple fringing reef, *f, f*:—it is a narrow platform of rock at the surface, dropping off at its edge to shallow depths, and then some distance out, declining more abruptly. Let the same island become submerged till II is the water line:—the reef extends itself upward, as submergence goes on, and may have the character at the surface represented by $b' f' b' f'$. There is here a fringing reef and also a barrier reef, with a narrow channel between, such as we have described as existing on the shores of Tahiti;* b' is a section of the barrier, c' of the channel, and f' of the fringing reef. Suppose a farther submergence, till III is the water line: then the channel (c'' c'') within the barrier is quite broad, as in the island of Nairai or Angau; on one side (f'') the fringing reef remains, but on the other it has disappeared, owing, perhaps, to some change of circumstance as regards currents, which retarded its growth, and prevented its keeping pace with the subsidence. With the water at IV, there are two islets of rock in a wide lagoon, along with other islets (i''' i''') of reef over two peaks which have disappeared. The coral reef-rock by gradual growth has attained a great thickness, and envelops nearly the whole of the former land. Nanuku, the Argo Reef, and Exploring Isles are here ex-

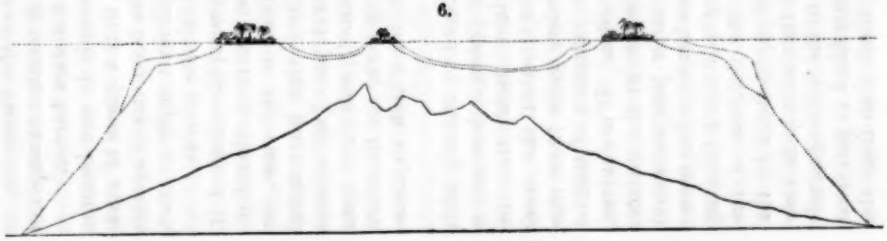
* This Journal, [2], vol. xi, p. 366.

MAPS AND IDEAL SECTIONS OF CORAL REEFS AND ISLANDS.

GAMBIER ISLANDS.







emplified, for the view is a good transverse section of either of them. b''' b''' are sections of the distant enclosing barrier, and c''' c''', and other intermediate spots, the water within.

The supposed similarity between these ideal sections and existing islands is fully sustained by actual comparison. Fig. 2 is a sketch of the island of Aiva in the Feejee Group. There are two peaks in the lagoon precisely as above; and although we have no soundings of the waters in and about it, nor sketches of peaks, facts observed elsewhere authorize in every essential point the transverse section given in figure 3, resembling closely, as is apparent, that in figure 1. The section is made through the line $b\,b$, $b'\,b'$, of figure 2. It is unnecessary to add other illustrations. They may be made out from any of the eastern groups of the Feejees, the Gambier Group of the Paumotus, or Hogoleu in the Carolines. Wallis's Island is another example of islets of rock in a large lagoon inclosed by a distant barrier.

It has been asked why the interior channels do not become filled by coral reef, as the island sinks, and thus a plane of coral result, instead of a narrow belt; and this has been urged against the theory of Mr. Darwin. But it is a sufficient reply to such an argument, to state the fact that the subsidence admits of no doubt, and that the islands referred to as exemplifications of it, present this very peculiarity. It should be received, therefore, as a consequence of it, instead of an objection to the view, for it is the most common feature with all islands that have broad reef-grounds, or in other words, that show evidence of subsidence during the growth of the reefs. Broad channels, and even open seas within, as in Nanuku and the Exploring Isles, are therefore to be received as results of the subsidence, for which explanations should be sought.

These explanations are at hand, and accord so exactly with facts ascertained, that the existence of inner passages becomes a necessary feature of such islands. It has been shown that the ocean acts an important part in reef-making;—that the outer reefs exposed to its action and to its pure waters, grow more rapidly than those within which are under the influence of marine and freshwater currents and transported detritus. It is obvious, therefore, that the former may retain themselves at the surface, when through a too rapid subsidence the inner patches would disappear. Moreover, after the barrier is once begun it has growing corals on both its inner and outer margin, while a fringing reef grows only on one margin. Again, the detritus of the outer reefs is, to a great extent, thrown back upon itself by the sea without and the currents within, while the inner reefs contribute a large proportion of their material to the wide channels between them. These channels, it is true, are filled in part from the outer reefs, but proportionally less from them than from the

inner. The extent of reef-grounds within a barrier, raised by accumulations at the same time with the reefs, is often fifty times greater than the area of the barrier itself. Owing to these causes the rate of growth of the barrier may be at least twice more rapid than that of the inner reefs. If the barrier increases twenty feet in height in a century, the inner reef according to this supposition would increase but ten feet; and any rate of subsidence between the two mentioned, would sink the inner reefs more rapidly than they could grow, and cause them to disappear. A wide flat reef, continuous over so extensive reef-grounds, could be formed only with an extremely slow rate of subsidence; and even then they would be liable to be cut up by the production of inner currents, destroying growing corals over the interior parts of the coral reef; so that whatever the rate of subsidence, the inner portions would grow less rapidly. There is therefore not only no objection to the theory from the existence of wide channels and open seas; on the contrary, their non-existence is incompatible with the mode of action going on. They afford the strongest support to the theory.

From these considerations it is evident that a barrier reef indicates very nearly the former limits as to the land enclosed. The Exploring Islets, (Feejee chart,) instead of an area of *six* square miles, the whole extent of the existing land, once covered *three hundred* square miles; and the outline of the former land is indicated by the course of the enclosing reef. A still greater extent may be justly inferred. For the barrier, as subsidence goes on, gradually contracts its area, owing to the fact that the sea bears a great part of the material inward over the reefs: and, consequently, the declivity forming the outer limit of the submarine coral formation, has a steep angle of inclination.

In the same manner it follows that the island Nanuku, instead of *one* square mile, extended once over *two hundred* square miles, or had two hundred times the present area of high land. Bacon's Isles once formed a large triangular island of equal extent, though now but two points of rock remain above the water.

The two large islands in the western part of the group, Vanua Levu and Viti Levu, have distant barriers on the western side. Off the north point of the former, the reef begins to diverge from the coast, and stretches off from the shores till it is twenty and twenty-five miles distant; then, after a narrow interruption, without soundings, the Asaua islands commence in the same line, and sweep around to the reef which unites with the south side of Viti Levu; and tracing the reef along the south and east shores, we find it at last nearly connecting with a reef extending southward from Vanua Levu. Thus these two large islands are nearly encircled in a single belt; and it would be doing no violence to principles or probabilities, to suppose them once to have formed

a single island, which subsidence has separated by inundating the low intermediate area. The singular reef of Whippey Harbor, p. 118, is fully explained by the hypothesis. We may thus not only trace out the general form of the land which once occupied this large area, (at least 10,000 square miles,) but may detect some of its prominent capes, as in Wakaia and Direction Island. The present area is not far from 4,500 square miles.

The whole Feejee Group, exclusive of coral islets, includes an area of about 5,500 square miles of dry land; while, at the period when the coral commenced to grow, there was, at least, as the facts show, 15,000 square miles of land, or nearly three times the present extent of surface.

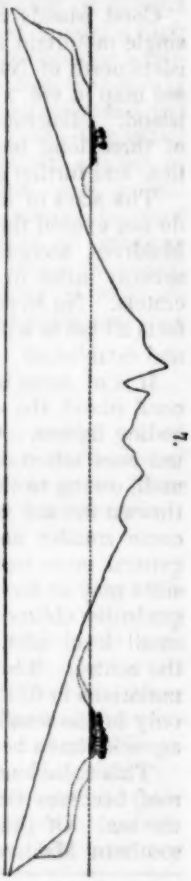
B. *Lagoons of Atolls.*—We pass from these remarks on the channels and seas within barrier reefs, to the consideration of the seas or lagoons of coral atolls. The inference has probably been already made by the reader, that the same subsidence which has produced the distant barrier, if continued a step further, would produce the lagoon island. Nanuku is actually a lagoon island, with a single mountain peak still visible; and Nuku-Levu, north of it, is a lagoon island, with the last peak submerged. This mode of origin may evidently be true of all atolls; for with the exception of the points of high land in the inner waters, there is no one essential character, distinguishing many of the Eastern Feejee islands from the Carolines to the North. The Gambier group, near the Paumotus, appears to have afforded the philosophical mind of Mr. Darwin the first hint with regard to the origin of the atoll; the contrast, and, at the same time, the resemblance, was striking; the conclusion was natural and most happy.* As some interest is connected with the history of new principles, and the illustration afforded is highly satisfactory, we have given a sketch of the Gambier group, (fig. 4, p. 189.) The very features of the coast,—the deep indentations,—are sufficient evidence of subsidence to one who has studied the character of the Pacific islands:† for these indentations correspond to valleys or gorges formed by denudation, during a long period, while the island stood above the sea.

The manner in which a farther subsidence results in producing the atoll, may be illustrated by fig. 5, p. 189. Viewing V, as the water line, the land is entirely submerged; the barrier, b'''', b'''', is an angular reef, enclosing a broad area of waters, or a *lagoon*,

* Captain Beechey, in his voyage in the Pacific, implies this resemblance, when he says of the Gambier group, which he surveyed, "It consists of five large islands and several small ones, *all situated in a lagoon formed by a reef of coral*,"—p. 120, Amer. ed. Balbi, the geographer, as Mr. Darwin remarks, describes those barrier reefs which encircle islands of moderate size, by calling them atolls, with high lands rising from their central expanse.—Darwin, op. cit., p. 41.

† This subject is discussed in the chapter, on the author's Geological Report, on the valleys of the Pacific islands. See also this Journal, [2], ix, 48.

with a few island patches of reef over the peaks of the mountains.* At a still greater subsidence (to the line VI) the islets, excepting one, have disappeared, owing to their increasing less rapidly than the barrier. The lagoon is in exact correspondence, in all its characters, with those of atoll reefs. Should subsidence now cease, the reefs, no longer increasing in height, would go on to widen, and the accumulations produced by the sea would commence the formation of dry land, as exhibited in figure 6, (p. 189.) Verdure may soon after appear, and the coral island is finally completed. It is not impossible that the land should form in certain favorable spots, while the subsidence is in progress if it be not beyond a certain rate.



The annexed figure represents the effect of a cessation or diminution of subsidence on the barrier reef, about a high island. The barrier reef has become a finished island, and forms a green belt to the land. The figure shows a section of such a belt.

All the features of atolls harmonize completely with this view of their origin. In form they are as various and irregular as the outlines of barrier reefs. Compare Angau of the Feejees, with Tari-tari of the Tarawan Group; Nairai or Moala with Tarawa; Nanuku with Maiana or Apamama. The resemblance is close; and in the same manner we might find all the forms of lagoon reefs represented among barrier reefs. We observe all those configurations which would be derived from land of various shapes of outline, whether the narrow mountain ridge, (as at Taputeouea, one of the Tarawan Islands,) or wide areas of irregular slopes and mountain ranges. Among the groups of high islands, we observe that abrupt shores may occasion the absence of a reef on one side, as on Moala; and a like interruption is found among coral islands. Many of the passages through the reefs may be thus accounted for.

The fact that the submerged reef is often much prolonged from the capes or points of a coral island, accords well with

* As the lagoon islets cover the summits of the subsided mountains, they afford the most favorable spots for reaching the rocks below by boring. In fig. 6, p. 189, the depth required for this purpose on an islet in the lagoon would be hardly a fourth what would be necessary on the enclosing reef.

these views. These points or capes correspond to points in the original land, and often to the line of the prominent ridge; and it is well known that such ridge-lines often extend a long distance with slight inclination compared with the slopes or declivities bounding the ridge on either side.

Coral islands or reefs often lie in chains like the peaks of a single mountain range:—for example, the sickle-shape line of islets north of Nanuku. Taritari and Makin, (Tarawan group, see map in vol. xii,) lie together as if belonging to parts of one island. Menchicoff atoll, in the Caroline Archipelago, consists of three long loops or lagoon islands, united by their extremities, and further subsidence might reduce it to three islands.*

The sizes of atolls offer no objection to these views, as they do not exceed those of many barrier reefs. Some of the larger Maldives, according to the crater theory, would require a crater seventy miles in diameter, with a rim made up of subordinate craters. No hypothesis of such extravagance is necessary. The facts all fall in with known principles, and are illustrated by known and established truths, without hypotheses of any kind.

It is of some interest to follow still further the subsidence of a coral island, the earlier steps in which are illustrated in the preceding figures. One obvious result of its continuation is a gradual contraction of the lagoon and diminution of the size of the atoll, owing to the fact already noted, that the detritus is mostly thrown inward by the sea. The lagoon will consequently become smaller and shallower, and the outline of the island in general more nearly circular. Finally, the reefs of the different sides may so far approximate by this process, that the lagoon is gradually obliterated, and the large atoll is thus reduced to a small level islet, with only traces of a former depression about the centre. Thus subsidence is connected with detritus accumulations in filling up the lagoon; and as filled lagoons are found only in the smallest islands, such as Swains and Jarvis, the two agencies have beyond doubt been generally united.

This subsidence, if more rapid than the increase of the coral reef, becomes fatal to the atoll, by gradually sinking it beneath the sea. Of this character evidently is the Chagos Bank.† The southern Maldives have deeper lagoons than the northern, fifty

* See Darwin on the probable disseverment of the Maldives, op. cit., p. 37, in which he points out indications of a breaking up of a large atoll into several smaller. A land with many summits or ranges of heights may at first have its single enclosing reef; but as it subsides, this reef contracting upon itself may encircle separately the several ranges of which the island consisted, and thus several atoll reefs may result in place of the large one; and further, each peak may finally become the basis of a separate lagoon island, under a certain rate of subsidence or variations in it, provided the outer reef is so broken as to admit the influence of waves and winds. The Maldives are a good example of this result. Some of the large atolls are properly atoll archipelagos.

† For a detailed account of this and other submerged reefs, see Darwin, p. 106.

or sixty fathoms being found in them. This fact indicates that subsidence was probably most extensive to the south, and perhaps also most rapid. The sinking of the Chagos Bank still further south, in nearly the same line, may therefore have some connection with the subsidence of the Maldives.

In view of the facts which have been presented, it appears that each coral atoll once formed a fringing reef around a high island. The fringing reef, as the island subsided, became a barrier reef, which continued its growth while the land was slowly disappearing. The area of waters within finally contained the last sinking peak: another period, and this had gone—the island had sunk, leaving only the barrier at the surface and an islet or two of coral in the enclosed lagoon. Thus the coral wreath thrown around the lofty island to beautify and protect, becomes afterwards its monument, and the only record of its past existence. The Paumotu Archipelago is a vast island cemetery, where each atoll marks the site of a buried island. The whole Pacific is scattered over with these simple memorials, and they are the brightest spots in that desert of waters.

ART. XV.—*Observations on the Freezing of Vegetables, and on the Causes which enable some Plants to endure the action of extreme Cold;* by JOHN LE CONTE, M.D., Professor of Natural Philosophy and Chemistry in the University of Georgia.

(Concluded from page 92.)

THE foregoing observations afforded no very satisfactory information in relation to the condition of the fluids of the proper *woody structure*:—they furnished positive evidence only with respect to the state of the juices contained in the succulent layers of the inner bark. During their progress, I endeavored to examine some transparent sections of the plants by means of a compound microscope; but it was found to be so difficult and unpleasant to carry on such observations in the open air, that I determined to make use of artificial cold in the prosecution of the investigation. With this view the following experiments were instituted; the frigorific agent being a mixture of snow and common salt.

For the pupose of testing the effects of cold, I selected the internodal portions of a vigorous freshly-cut elder stalk (Sambucus canadensis), of about *one inch* in diameter: the diameter of the pith was nearly *one-half* that of the stalk. The pith of this plant is surrounded by a zone about one-twentieth of an inch in thickness, which is more succulent than the central portions of the pith proper, and from which fluid could be readily pressed by lateral compression *in situ* with the back of a knife. A smaller,

but sensible, quantity of fluid could, likewise, be expressed from any portion of the pith, so as to appear on the cut surface, and which would disappear as soon as the compressing force was withdrawn. It was also found, that fluid appeared at the transverse cut surface of the liber, when pressed against the subjacent wood at the extremities. It was hoped, that the application of the foregoing tests, would enable us to ascertain with considerable certainty, whether the juices of these portions of the plant could be readily frozen, independently of any light which microscopic examination might throw upon the question.

Experiment No. 1.—February 2nd, 1851, 1 o'clock, P. M.; the temperature of the room being 42° Fahr., and that of the freezing mixture varying, during the experiment, from 2° to 5° Fahr. With the view of excluding the direct contact of the frigorific mixture, the internode of elder was inserted into a water tight tinned sheet-iron case or cylinder, of nearly the same diameter as the stalk,—the whole was plunged vertically, and in the natural growing position, into the freezing mixture; about seven-eights of the internode being below the surface. At the expiration of *two hours*, the stalk was removed from the case for examination. The extremity which was above the cold mixture, was found to be surrounded by a thin transparent coating of ice. The fluid seemed to have been forced up along the inner bark and the succulent zone surrounding the pith, by the contraction which took place in the part of the stem below the mixture, before congelation supervened. The *lower* extremity was found to be quite free from external ice, and appeared comparatively dry. The fluids of the bark, as well as those of the succulent zone around the pith, were obviously *congealed*. The frozen condition was rendered evident, by slicing the bark longitudinally with a sharp knife,—the ice could be scraped off with the edge of the instrument. In like manner, small masses of solidified juice could be detached from the succulent zone as well as the pith.

Under the microscope,—when a transverse section was made with a knife which was artificially cooled, and placed on a plate of glass which was cooled in the same manner,—the process of thawing was quite conspicuous and beautiful, especially in the succulent zone. In the woody structure, the process of liquefaction was manifested by the rapid appearance of *bright apertures*, which were mere translucent points when first placed under the glass. Slices of wood which had not been subjected to the influence of cold, presented *no changes* under the magnifier. As it is necessary to make the slices very thin, in order to secure the requisite degree of transparency, the thawing takes place with great rapidity. The observations must, therefore, be made quickly:—the knife with which the slices are made, as well as the glass plate on which they are placed for examination, should be artificially cooled.

Experiment No. 2.—In the previous experiment, the lower extremity of the internode of elder was exposed to the action of the cold, and it might be supposed, that the frigorific influence was propagated along the stalk *longitudinally*, the direction in which the experiments of MM. Aug. De La Rive and Alph. De Candolle show, the conducting power of wood to be the greatest. For the purpose of obviating this difficulty, a hollow tin cylinder, six inches in length, open at both extremities, was passed transversely through the centre of the opposite sides of a small wooden box. Another recently cut internode of elder was thrust through the cylinder, having both extremities projecting several inches beyond the ends of the metallic case, so that, only the *middle portions* would be subjected to the frigorific influence. On the 6th of Feb., 1851, at 1 o'clock P. M., the temperature of the room being 37° Fahr., the freezing mixture was introduced into the box. At the end of *two hours*,—the temperature of the mixture varying from 2° to 6° Fahr.,—the stalk was withdrawn for examination. The bark in the middle of the part which had been exposed to the cold, was *found frozen* as in the previous experiment; neither the bark, succulent zone, pith or wood at the *extremities*, exhibited any indications of congelation of the fluids contained in them. The middle of the stalk was then cut through by means of a small saw which had been artificially cooled. On examining the transverse section, the liber and succulent zone around the pith were evidently frozen; portions of solidified sap could be removed with the point of a knife. Under the microscope, the thawing of thin transverse slices of the woody structure, was manifested by a series of changes identical with those observed in the first experiment. Indeed, the physical properties of the wood were sufficiently modified to indicate the frozen condition by the use of a cutting instrument:—it was harder than natural, and the section presented a *polished glassy appearance*. At the same time, a portion of the branch of a *Pinus tæda* was subjected to the same degree of cold. The microscope revealed the fact, that its juices were, likewise, frozen,—a similar change, indicative of thawing, being clearly observable.

Experiment No. 3.—The foregoing experiments were tried on portions of plants which had been recently cut from the parent stem or branch, and it might be imagined that the case would be different with a *growing* plant. For the purpose of testing the validity of this idea, two vigorous shoots of the Ailanthus,—which had been planted in a large box of earth,—were, on the 19th of Feb., 1851, submitted to experiment. The temperature of the room was 56° Fahr., that of the freezing mixture varied from 0° to 5° Fahr. Tin tubes were placed around the base of the stems, so as to protect them from the immediate contact of

the mixture. The frigorific material was kept around each tube by means of two small wooden boxes, which were perforated at the centre of the bottom, so as to permit the tubes and contained stems to pass vertically through. At the expiration of *four hours*, the cold mixture was removed from one of the shoots, and it was cut through the middle of the part which had been exposed to the cold. The juices of the bark and wood were evidently frozen. This was rendered obvious by placing transparent slices of them under the microscope, as in the previous experiments. It was, likewise, proved by the fact, that, at first, when cut with a cold knife, none of the juices were forced out by the passage of the cutting instrument; but after a short time, when thawing had taken place, any attempt to slice the wood transversely would render the presence of the fluids manifest on the cut surface. The *other shoot* was allowed to remain undisturbed for *six hours* after which, the frigorific mixture was removed, and the box of earth in which it was planted placed in the open air, for the purpose of ascertaining what effect the cold might have on its *vitality*. About the middle of March, the buds began to swell, and in due season, the leaves were developed. The shoots of Ailanthus on which these experiments were tried, were suckers of two years growth.

From the preceding experiments, two conclusions seem to be legitimately deducible. *First*, that the sap of certain plants *can be* readily frozen by the application of a comparatively *moderate* degree of cold. For, although, the temperature of the frigorific mixture varied from 0° to 6° Fahr., yet, as there was a metallic tube surrounding the stem, the temperature of the latter must have been considerably higher. Direct observation proved this to be the case: a thermometer introduced into the metallic case never indicated a temperature lower than 15° Fahr. It must also be borne in mind, that as the *duration* of the frigorific action was never more than from *two to four hours*, the *central* portions of the stems submitted to experiment, must have possessed a temperature notably *higher* than the exterior parts: and yet, the structures around the *pith* were found congealed. Moreover, a degree of cold equal to zero of Fahrenheit is quite *moderate* in comparison with the natural cold endured by the very plants on which the experiments were performed. And *secondly*, that congelation of the juices of vegetables *does not*—as many physiologists imagine,—necessarily and inevitably result in the death of the whole plant, or of the *part* in which it takes place; but, on the contrary, that frequently, *no injurious consequences follow*.

With such a state of facts before us, we should naturally expect, that in high latitudes, the sap of all perennial plants must be frozen during several of the winter months. *Such I believe to be the fact.* Robert Boyle informs us, on the authority of Capt.

James, that at Charleston Island (now called Charles Island) in Hudson's Bay, the trees had to be *thawed by fire before they could be cut down.** I have been credibly informed, that the "lumberers" of Maine and New Hampshire are familiar with the fact, that during periods of extreme cold, the sap of many of the forest trees becomes so frozen, that the physical qualities of the wood are altered to such an extent, *that it is difficult to cut it with an axe.* M. Duhamel observes, that the maples in Canada, where the frost is long and severe, begin to bleed, when wounded, with the first thaw, and stop again when it freezes; and that this, in frosty days, occurs only on the south side of the tree.† In all these cases, it is sufficiently obvious, that the freezing of the sap could not have been fatal to the life of the trees; for, in that event, a single severe winter would destroy every plant.

But, perhaps, it may be granted, that the foregoing experiments and observations, are sufficient to prove, that the juices contained in the *aërial or ascending stems* of many plants may be frozen without destroying the power of vegetation; and yet, it may be asked, does not the complete congelation of the fluids included in *all parts of the vegetable*, comprehending the root or descending axis and its appendages, *necessarily* result in the destruction of its vitality? Even the admission that *large portions of the aërial stem* may be frozen with impunity, impresses the character of error upon the commonly-received opinion among many of the most eminent phytologists; but, I think, facts are not wanting to show, that the juices contained in *every part of some plants*, may be congealed without injury to the powers of vegetation. Although Leopold von Buch and many other philosophers, even as late as 1825, were disposed to reject the statement of the elder Gmelin, that the ground is perpetually frozen in the northern parts of Siberia; yet, the fact has been abundantly corroborated in our days by the observations of MM. Adolph Erman, von Humboldt, Hansteen, Schergin and others. At the town of Yakutsk, in latitude 62°, M. Schergin attempted to sink a well and was about to abandon the project in despair of obtaining water, when Admiral Mangel persuaded him to continue his operations until he had perforated the whole stratum of ice. This he did, and kept a complete journal of the work. The well was dug to the depth of nearly 400 feet, with the following most remarkable results as to the temperature of the ground:‡

50	English feet,	.	.	18°·5 Fahr.
77	"	"	.	. 19 ·6 "

* Vide Boyle's works, vol. ii, p. 274. London, 1744.

† Physique des Arbres, vol. ii, p. 258.

‡ Vide Erman, Comptes Rendus de l'Acad. des Sciences, tome vi, p. 501, 1838; also von Baer, in Edinb. New Phil. Journ., vol. xxiv, p. 435, 1838.

119	English	feet,	.	.	22°·	Fahr.
217	"	"	.	.	27 ·5	"
305	"	"	.	.	28 ·6	"
382	"	"	.	.	31	"

From which it appears, that the soil at this place is frozen to the depth of about 400 feet. Observations kept at Yakutsk for a series of years, show that the mean temperature of the year is nearly 14°·5 Fahr.* In winter, a degree of cold exceeding –58° Fahr. takes place every year. The mean temperature of two winter months is below –40° Fahr.; so that mercury is solid for *one-sixth* of the year, and has been known to remain frozen during *three consecutive months!*† A comparatively warm summer is joined to this cold winter; the mean temperature of June, July and August being 56°·75, 65°·75 and 61°·25 Fahr. respectively, and the maximum heat of the summer day is sometimes as great as 77° Fahr. During the 128 days of the year in which there is no frost at Yakutsk, the strata of eternal ice are never thawed to a greater depth than *three feet*, and yet, "vegetable life continues not merely uninjured, but is favored in the highest degree by the equable and very rapid increase of heat."‡ This is not restricted to *annual* plants; for M. Erman informs us that noble larch forests (Pinus larix) flourish on the east side of this town, where their roots rest upon inferior strata of perpetual frozen earth, and where, even the superficial stratum in which they are imbedded, is frozen for nearly *eight months of the year!* As it is physically impossible that the roots of these trees can penetrate deep enough even to *approach* the stratum of *invariable temperature* in this high latitude, the *whole plant*, including the descending axis and its appendages, must, during the winter, be exposed to a degree of cold considerably below the zero of Fahrenheit.

Is it possible to imagine, that the sap of these trees remains unfrozen in a climate where mercury is solid during *one-sixth* of the year, and where the stratum of perpetual ground-ice extends to the depth of 400 feet? Here we have an *extreme case*, in which it is sufficiently obvious, that no known combination of vital and physical causes could prevent the fluids contained in these plants from undergoing congelation every winter: the conclusion seems, therefore, to be unavoidable, that *they are actually*

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* Vide M. Mahlmann's table of mean temperatures, in Kaemtz's Complete Course of Meteorology. English Translation by C. V. Walker. London, 1845, p. 177. Erman puts down the mean temperature of this place at 18°·5 Fahr.

† Vide Adolph Erman's Travels in Siberia, translated from the German by W. D. Cooley. Philadelphia, 1850, vol. ii, p. 278; also Comptes Rendus, (Paris) tome vi, p. 502.

‡ Erman. Op. cit. supra., vol. ii, p. 279.

frozen at this period of the year, and, consequently, that such an accident *is not always injurious* to vegetation.*

Neither is the example of Yakutsk an isolated one: for a great part of the vast territory of Asiatic Siberia, and a smaller portion of Europe and North America, lying north of the isothermal line of 32° Fahr., support extensive forests of birch, (Betula alba,) Norway spruce, (Pinus abies,) larch, (P. larix,) Cembra pine, (P. cembra,) Scotch fir, (P. sylvestris,) white spruce, (P. alba,) American silver fir, (P. balsamea,) and black spruce fir, (P. nigra,) where the ground-ice is perpetual.† These facts appear to warrant the conclusion of M. Adolphus Erman, namely: that it is now fully established, "that many arborescent plants require as the condition of their thriving, only the summer heat and the humidity of the air that suits them; and that they are, therefore, not only quite insensible to the rigor of winter, but, in spreading over the plains and mountains of the earth, *are wholly independent of the temperature of the ground or mean temperature.*"‡ The development of leaves and vegetation depends less on the temperature of the soil, than on that of the air in the spring and summer: it only requires that the ground should be so far thawed, that the tree may be able to draw from it sufficient moisture for its growth. This is especially true of several species of the coniferæ; for many of them are enabled to brave the most rigorous winters, and pass through all the phases of flowering and fructification, provided the summer be hot enough, and of sufficient duration.

But it may be objected, that when the sap contained in the trunks of trees becomes frozen, it *cleaves* them with a great noise, —a phenomenon not uncommon in high latitudes,—and that, therefore, the fact that the majority of them *are not* thus cleft, *proves that their juices have not been congealed.* It is proper to remark, *en passant*, that even in the cases where the trunks of trees *are split* by cold, *it does not always result in the death of the plant;* although the wood is generally rendered unfit for the purposes of timber. This effect has been, almost universally, ascribed to the expansion which the sap undergoes during the process of congelation;—a force abundantly adequate to produce such a result, *provided all parts of the tree were perfectly rigid*

* The lowest temperature that has yet been observed on the earth, is probably that noted by Neveroff, at this place (Yakutsk) on the 21st of January, 1838. "The instruments used in this operation were compared with his own by Middendorff, whose operations are always conducted with extreme exactitude. Neveroff found the temperature on the day above-named, to be −76° Fahr." (Humboldt's Cosmos, (Bohn's Scientific Library,) translated from the German by E. C. Otté, vol. iii, p. 43. London, 1851.) The maximum natural cold previously recorded, is −70° Fahr.; being that observed by Capt. Back at Fort Reliance in January, 1834.

† Vide Johnston's Physical Atlas.

‡ Vide Travels in Siberia. Ed. *cit. supra*, vol. ii, p. 360.

and unyielding. As it is manifest, that this condition ***does not*** obtain in any tree, I am disposed to think that this opinion is erroneous, and that the effect here referred to, ***is not the result of the freezing of the fluids***, and consequently, is not a necessary accompaniment of that phenomenon. The following considerations have led me to refer the cleaving of trees by cold, ***to the unequal contraction which takes place in the trunk*** (usually after the complete congelation of its juices) ***in consequence of a sudden depression of temperature.***

1. Observations show, that the ***oldest and largest*** trees are usually those which are cleft; while the younger trees exhibit no such effects. In the account given by J. Evelin of the effects of the great frost which occurred in England during the winter 1683–4, we are informed, that "the rifting so much complained of, has happened chiefly among the overgrown trees, especially oaks," whereas, elms of only twenty-five or thirty years standing, were untouched.* Now, if ***freezing of sap*** is the cause of the phenomenon, young and small trees would certainly be more liable to be cleft; both on account of the greater accessibility of their interior parts to the exterior atmospheric changes, and the presence of a greater amount of fluid in their tissues. On the contrary, if the effect is produced by the ***contraction*** of the exterior layers of wood, we should naturally expect the rifting to take place in old trees, where the heart wood is indurated and of course unyielding.

2. The same effect is produced by extreme cold on ***dry and seasoned timber.***† At Prince of Wales' Fort on Churchill river near Hudson's Bay, Capt. Middleton noticed that ***trees, joists and rafters*** were burst with great noise from the effects of cold.‡ Near Moscow the timber-work of houses is frequently observed to crack during severe winters.§ Other instances might be cited; but the effects of cold in temporarily widening the small cracks produced by ordinary desiccation in posts and pillars, must be so familiar to every one, that farther notice is unnecessary. Is it reasonable to suppose, that in these cases, the splitting is caused by the congelation of the comparatively small amount of hygrometric moisture which is known to be present in the best seasoned wood? On the other hand, is it not obviously a phenomenon of unequal contraction, precisely analogous to the smaller fissures produced by rapid desiccation?

3. All travellers in high northern latitudes, testify that the very rocks are sometimes rent asunder by the intensity of the cold, and that the ground is frequently cleft by the same cause,

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* Vide Phil. Trans., vol. xiv, p. 559. Abridged, vol. ii, p. 153.
† Vide Phil. Trans., vol. xiv, p. 559.
‡ Phil. Trans., vol. xlii, p. 157.
§ Boyle's works, vol. ii, pp. 274 et 276. London, 1744.

producing openings many yards long and ten or twelve inches wide. Such phenomena cannot be ascribed to the expansive power of freezing water, because they occur during mid-winter, and in latitudes where the rigor of the climate is such, that the earth is frozen 300 or 400 feet deep, (certainly far below the depth of these superficial fissures,) long before these effects are observed. They are unquestionably the result of superficial contraction produced by the sudden application of intense cold, and are precisely analogous to the fissures which originate in clay and mud during the process of drying.

4. Ice itself is frequently known to crack from the same cause. Of this character were the fissures in the ice—some of them *four inches* wide—which M. Erman observed near Bol-Atluimsk on the Obi and at Posolskoi. He very correctly ascribes them to the cooling and contraction of the upper stratum of ice *subsequent to its perfect congelation.** Such facts are matters of the most common observation in all climates where the cold is of sufficient intensity to maintain water in a solid condition for any considerable portion of the winter. The cracking always takes place during periods of rapid augmentation of cold. To those who may be incredulous as to the *adequacy* of superficial *contraction* to produce the observed effects, it may be proper to state, that according to the experiments of MM. Brunner and Schumacher, the contraction of ice consequent upon a diminution of temperature, is greater than that of any other solid body hitherto examined. The former found the amount of linear contraction to be equivalent to ·00002083 for a degree of Fahrenheit; the latter obtained a still higher result, viz: ·000029086.† It is well known that glass, in which the coefficient of dilatation is very small, will readily crack by the sudden application of cold.‡

The facts and considerations above detailed, seem to point to the unequal contraction produced by the sudden application of cold, as the true cause of the bursting of trees in rigorous climates. Whatever may be thought of the universality of this cause, direct experiments and observations prove, that congelation of the sap *per se, does not* invariably produce the effect. It appears to us infinitely more probable, that the rifting supervenes subsequent to complete congelation of the juices: first, because the *increased non-conducting power* of the woody structures under these circum-

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* Erman's Travels in Siberia. Ed. *cit. ante.* vol. i, p. 331, et vol. ii, p. 218.

† Vide Ann. de Chim. et de Phys., 3d series, vol. xiv, p. 369—1845, as cited in Silliman's Journal, 2d series vol. i, p. 117—1846; also vol. iii, p. 450 of the same Journal.

‡ In two recent series of experiments, independent of each other, MM. Pohrt and Moritz have found the linear expansion of ice for an interval of *one degree* of Fahrenheit's scale, to be 0·0000285 and 0·000028778: fractions nearly accordant with the results of Schumacher's experiments. (Vide Liebig and Kopp's Report on the Progress of Chemistry, etc. English Translation, vol. i, p. 44. London, 1849.)

stances would, of necessity, tend to establish a greater *inequality* of contraction, when the frigorific influence began to operate, and, therefore, a greater liability to rupture; and secondly, because the greater *rigidity* of the interior parts of a tree whose juices are frozen, would tend to bring about the same result upon a sudden reduction of temperature. The reason why some species of trees are more liable to be rifted by cold than others, is, probably, attributable to a difference in the compressibility of their structures when in a frozen condition.

But it may be asked, if the *freezing of the sap* does not always kill plants, in what manner does cold produce death in vegetables? As this is a point which has been investigated by Göppert, Morren, Lindley and others, I do not propose to notice it farther, at this time, than to indicate the learning of their deductions on the subject under consideration. The observations of MM. Göppert and Morren seem to prove, that the common opinion, that cold acts mechanically upon the tissues of plants, by expanding the fluid they contained and bursting the cells or vessels in which it is enclosed,—is totally untenable; such supposed laceration of the vegetable tissues seldom, if ever, taking place even when the most succulent plants are frozen and killed by cold. During the process of congelation, each cell of the tissue becomes individually larger, by the augmentation of volume which attends the solidification of the contained fluid; but there is *no bursting*, because the membrane is *extensible*, and when thawed, the cell recovers itself by its elasticity. The more recent observations of Professor John Lindley, are, on the whole, confirmatory of these conclusions. For although, in some instances, he found the tissue of the succulent parts of plants lacerated, as if by the dilation of the fluid it had contained; yet, this result was by no means an invariable concomitant of freezing, and it is not essentially connected with the destruction of vegetable life. Upon a careful review of all the facts, Prof. Lindley concludes, "that the fatal effects of frost upon plants is a more complicated action than has been supposed; of which the following are the more important phenomena:—

"1. A distension of the cellular succulent parts, often attended by laceration, and always by a destruction of their irritability.

"2. An expulsion of air from the aeriferous passages and cells.

"3. An introduction of air, either expelled from the air passages or disengaged from the water during the act of freezing, into parts intended exclusively to contain fluid.

"4. A chemical decomposition of the tissue and its contents, especially the chlorophyll.

"5. A destruction of the vitality of the latex and a stoppage of the action of its vessels.

"6. An obstruction of the interior of the tubes of pleurenchyma (woody fibre) by the distension of their sides."*

It will be observed, that the phenomena are partly mechanical, partly chemical, and partly vital. So far as the mechanical effects are concerned, it is very plain, that whatever increases the amount of moisture in the plant, augments the liability to laceration of tissues, when freezing supervenes. In relation to the chemical and vital phenomena, it is sufficiently obvious, that the effects of cold must vary with the condition of the fluids in the plants. The well-known evil influence of cold in the spring, or after a warm spell in winter, is, probably, referable to the augmented *susceptibility* which seems to attend the *growing state*. It is difficult to say whether this increased susceptibility to the action of cold, is due to an alteration in vital sensitiveness, or to a proneness in the fluids to enter into chemical decomposition.† Possibly, both causes may be in operation: but additional observations and experiments are wanting to clear up many points relating to the *exact manner in which the death of plants is caused by cold.* Nevertheless, whatever may be our degree of ignorance in relation to these points, it is hoped that we have succeeded in establishing the fact, *that the destruction of life is not an invariable concomitant of the congelation of the juices of plants;* and consequently, that they *have not* the relation of effect and cause. Indeed, the proof of this may be considered two-fold; *both negative and positive.* For it is well known that many tropical exotics are destroyed by a degree of cold considerably *above* the freezing point of water, when, of course, their sap cannot be in a state of congelation: while, on the other hand, as I have endeavored to show in this memoir, the juices of other plants are obviously and repeatedly *frozen* without the slightest injury to the powers of vegetation.

The analogy between animals and vegetables seems to be, in this respect, almost as perfect as it is remarkable. A degree of cold which absolutely freezes the fluid contained in their structures *is not* equally fatal to all plants. As among animals, each species of plant is adapted to endure a certain range of temperature, which determines, with more or less precision, the limits of its geographical distribution. The fact that vegetables are less susceptible to the injurious influence of cold when in a *dor-*

* Vide Prof. A. Gray's Abstract of Prof. Lindley's Memoir, in Silliman's Journal, 1st series, vol xxxix, p. 18, et seq.—1840.

† It is very probable that the injurious effects of cold upon *growing* plants, may be, in part, due to the sudden stoppage of the changes which attend the process of active cell-development. It is reasonable to suppose, that any disturbance of this process at such a period, would lead to rapid and fatal chemical reactions. The well-known fact that, in frozen potatoes, all the starch is converted into dextrin and sugar, shows that important chemical changes may be brought about by the influence of cold.

mant state, seems to be a wise and inestimably excellent provision appointed by nature for the preservation of the vitality of the system against the extreme cold of winter. Observations and experiments are yet wanting to determine, whether those members of the animal kingdom, which have little or no power of resisting external changes of temperature, are endowed with a like *increased* immunity from the injurious effects of cold, during the period of hybernation. On a future occasion, I hope to make this point a subject of special investigation.

University of Georgia, July 4th, 1851.

ART. XVI.—*On the Compound Ammonias, and the bodies of the Cacodyle Series;* by T. S. HUNT, Chemist to the Geological Commission of Canada.

THE beautiful researches of Hofmann and Wurtz have shown the existence of a large class of organic alkaloids closely related to ammonia, which have already been noticed at different times in this Journal.* As regards their composition, we will only recall that in the alkaloids of Wurtz, the elements of an equivalent of ammonia are united with those of a carbohydrogen, CH_2, C_2H_4 C_5H_{10} or what is the same thing, that CH_3, C_2H_5 and C_5H_{12}, the so-called radicals, methyl, ethyl, and amyl, may be regarded as replacing an atom of hydrogen in ammonia. Hence, as we have before remarked in speaking of them, "they sustain to their corresponding alcohols the same relation that ammonia does to water.† Water, as we have on more than one occasion shown, is not only the analogue, but the strict homologue of the alcohols, so that the molecule H_2 is the equivalent (homologue) of C_2H_6 and its homologues, and H of ethyl, methyl and amyl.‡ The class of bodies under consideration presents some interesting illustrations of this relationship.

Mr. Hofmann has been able by the action of ammonia upon hydrobromic and hydriodic ethers, to form directly the corresponding salts of the new alkaloids, and these alkaloids, with other equivalents of the ethers, have yielded him compounds in which two and three equivalents of hydrogen are replaced by the same or by different carbo-hydrogens; so that representing C_2H_5 by Et, the final result of the action of ammonia is NEt_3, which is still an alkaloid. Other carbo-hydrogens not homologous with ethyl may be introduced, and Hofmann has obtained alkaloids containing one and two equivalents of phenyl C_6H_5, with one or more of ethyl.

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* See [2], vol. ix, p. 66; xii, pp. 115, 428.

† This Journal, vol. ix, p. 65.

‡ Ibid, and vol. v, p. 265.

Although ammonia and its derived alkaloids form with acids, salts analogous to those of the inorganic bases, they must be distinguished from oxyds like Zn_2O, inasmuch as they unite directly with HCl and NHO_3, while the oxyds yield salts only by the elimination of water; in chlorid of ammonium it is the hypothetical NH_4, which represents Zn in the chlorid of zinc. The analogy between Zn_2O and H_2O leads us to suppose the possibility of such a compound as the oxyd of ammonium which would be formed by a direct union of ammonia with the elements of water. But such compounds, if they exist, are very unstable; and as the alkaloids are either readily disengaged from their aqueous solutions by heat, or else are insoluble in water, it is very difficult to prove the existence of these oxyds. If, however, an alkaloid could be made to unite with a homologue of water, the elements corresponding to H_2O might form a more stable combination, and the reality of the action be established. Such a result has actually been attained by Mr. Hofmann, who has indirectly formed a combination of triethammine with alcohol. An ammonia uniting with water which has two atoms of replaceable hydrogen, might form either $NH_3O=NH_4$, HO or $N_2H_6O=(NH_4)_2O$. Did triethammine unite directly with hydric ether, we might obtain the alcohol compound corresponding to the latter oxyd, but alcohol is Et HO containing but one atom of C_2H_5, and consequently we have NEt_4, HO. It is obtained by the action of triethammine upon iodid of ethyl, which is the homologue of hydriodic acid; and as the acid produces with ammonia the iodid of ammonium, the ether yields the iodid of the new quasi-metal *tetrethylammonium*, which, when decomposed by oxyd of silver, yields the hydrated oxyd of the new base $(NEt_4H)O$, corresponding to (KH)O, hydrate of potash, which it closely resembles in its acridness, causticity and powerfully alkaline characters, particularly as shown in its reactions with metallic salts, and in its power of saponifying oils. Although termed an organic alkaloid, it will be seen that this and its analogous compounds cannot be assimilated to the organic bases containing oxygen like quinine, with which they have been compared, as the latter combine directly with acids and carry their oxygen into their saline combinations, while Hofmann's bases eliminate an equivalent of water which contains their atom of oxygen.

The action of an alloy of potassium and antimony upon the iodid of ethyl, has furnished to MM. Löwig and Schweizer a volatile liquid, spontaneously inflammable and having the formula $C_6H_{15}Sb$, which corresponds to triethammine, in which N is replaced by Sb.* It does not appear whether it forms direct

* Jour. für Prakt. Chem., t. xlix, p. 385, t. l, p. 321, and this Journal, vol. x, p. 263.

compounds with acids. When slowly oxydized it takes up an equivalent of oxygen and yields a viscid liquid which combines with acids, and forms salts with the elimination of an equivalent of water. M. Gerhardt has shown that this compound, which the authors designate as *oxyd of stibethyl*, is to be regarded as the hydrate of a new base, $C_6H_{13}Sb$, for which he proposes the name of *stibethine*;* it is formed from stibethyl by the loss of H_2, as harmine is derived from harmaline. The constitution of the hydrate is analogous to Hofmann's new ammonium bases; SbC_6H_{13}, $H_2O=(SbC_6H_{14}, H)O$, and SbC_6H_{14} is equivalent to NH_4, ammonium. Nitric acid oxydizes H_2 in stibethyl and forms an acid nitrate of stibethine. Sulphur, chlorine and bromine combine directly with stibethyl, yielding compounds which have all the characters of salts of stibethine and may be formed by double decomposition from the salts of the oxyd. Stibethyl even decomposes strong hydrochloric acid, evolving hydrogen to form a chlorid which is also produced by the action of a metallic chlorid upon the nitrate of stibethine; its composition is that of an acid salt, SbC_6H_{13}, $2HCl = SbC_6H_{14}$, Cl, HCl. It reacts like a chlorid of potassium or sodium with metallic solutions, and forms with sulphuric acid a sulphate with disengagement of hydrochloric gas.

More recently M. Landolt has obtained the methyl compound analogous to stibethyl by a similar process.† It corresponds to trimethammine, being $SbC_3H_9=SbMe_3$, and yields a series of compounds like those of stibethyl. When placed in contact with iodid of methyl, an energetic combination ensues, and a crystalline product is obtained which is $SbMe_4I$, the equivalent of Hofmann's iodid above described. Decomposed by oxyd of silver, the hydrated oxyd of the new base, which the author calls *stibmethylium*, is obtained; it closely resembles the oxyd of tetrethylammonium, and its salts are said to be isomorphous with those of potash. The author writes the formula of the iodid as above, and the oxyd $SbMe_4O$; this is evidently an error, the oxyd will be $(SbMe_3H)O$, like the corresponding nitrogen compound. He has observed that stibethyl yields similar compounds with iodid of ethyl, and with iodid of methyl, the analogous body $(SbEt_3Me)I$.

With these results before us, we are ready to inquire into the constitution of the bodies of the cacodyl series. It must be observed that the elimination of H_2 is characteristic of the alcohols, as is seen in the formation of aldehydes and acids, and they seem to preserve this same character in their combinations; thus the fourth ethyl atom which is combined with triethammine is decomposed at the temperature of boiling water into C_2H_4 and H_2O.

* Comptes Rendus des Travaux de Chimie, 1850, p. 400, and this Journal, vol. xii, p. 430.

† Ann. der Chem. und Pharm., lxxviii, p. 91.

Let arsenic replace nitrogen in Wurtz's *ethammine* $NC_2H_7 = NC_2H_5, HH$, and we have AsC_2H_7, from which if H_2 be abstracted, there remains $AsC_2H_5 = AsC_2H_3, HH$, a new base corresponding to stibethine; such a base is contained in the chlorid of cacodyl, and has been recognized by M. Gerhardt under the name of *arsine*,* of which the hydrochlorate and hydrobromate, are Bunsen's chlorid and bromid of cacodyl. The compound analogous to the oxyd of stibmethylium will be $(AsC_2H_5)_2, H_2O$ or $C_4H_{12}As_2O$, (equivalent to K_2O,) which is *alcarsine*. The relation between the oxyd and the chlorid is evident; it is difficult to believe that the bodies described by Bunsen as oxychlorid and oxybromid of cacodyl are any thing else than mixtures of alcarsine with hydrochlorate and hydrobromate of arsine, and the more so, as their composition after his analyses does not seem to be well defined. M. Bunsen did not analyze the compounds of alcarsine with oxygen acids; indeed only the sulphate seems stable; it is acid and deliquescent, and is probably a bisalt—the bisulphate of arsine.

The sulphuret of cacodyl is analogous to the hydro-sulphuret of ammonia, and arsine sustains to alcarsine the same relation as ammonia to oxyd of ammonium. It is worthy of notice that there are two conditions of alcarsine, the one a fuming and spontaneously inflammable liquid, and the other, formed during the slow oxydation of the first, a viscid, syrupy substance, comparatively indifferent to chemical agents, and but difficultly oxydized: —the viscid, inactive form corresponds to stibethine. Researches upon this variety of alcarsine and its salts would be very desirable. The compounds resulting from the action of chlorid of mercury and nitrate of silver with alcarsine, are probably compounds of arsine, analogous to the ammoniacal combinations of these salts. It is to be remarked that while the salts of stibethine are, from the very mode of their formation, acid, those of arsine, if we except perhaps the sulphate, are neutral.

Cacodyl is formed by the reduction of the hydrochlorate of arsine, *chlorid of arsenium*, by zinc; precisely as $2ZnCl + K_2$ give $2KCl + Zn_2$, we obtain chlorid of zinc with the elimination of arsenium, that is, of $AsC_2H_6 + AsC_2H_6 = C_4H_{12}As_2$. Cacodyl is thus precisely analogous to a metal, and with chlorine or sulphur yields compounds of the arsine series; the above formula however, represents two volumes of vapor, while the equivalent of the chlorid is represented by AsC_2H_6, Cl. I have however endeavored on a previous occasion to show that the atom of the metals in their free state is represented by M_2, and hence cacodyl corresponds perfectly to Zn_2, which in combining with chlorine, breaks up to form two equivalents of $ZnCl$; *alcargen*, cacodylic

* Prècis de Chimie Organique, vol. i, p. 389.

acid is not an oxyd of cacodyl, for its formula is $C_2H_5AsO_2$, and being anhydrous it is equivalent to a compound of ammonia with oxygen, and not of ammonium as M. Bunsen's theory demands.

MM. Löwig and Schweizer assert that by oxydation, stibethyl yields $C_4H_5Sb{=}SbEt$, which combines with O_5, S_5. As we have no other evidence that the type of the ammonia is ever thus destroyed, it is more probable that the action removes H_2 from one atom of Et, as in the formation of stibethine, and oxydizes the two remaining atoms of ethyl, leaving H in their place; $Sb, C_2H_5 = Sb, C_2H_3, HH$, corresponding to arsine, and like it combining with O_2, S_2. (O_5 in their notation not being divisible by two, is inadmissible, unless the formula is to be doubled.) The properties of the new compound, *stibethylic acid* of the author, and $C_4H_5SbO_5$ in his notation, but more probably $C_2H_5SbO_2$, lead us to conclude that it is the antimonial species corresponding to alcargen, $C_2H_5AsO_2$. It is a white solid, soluble in water and alcohol but insoluble in ether, and is converted by H_2S, into an odorous compound, in which its oxygen is replaced by sulphur; the history of the body is not however complete.

I remarked four years since, that glycocoll is the nitrogen species corresponding to alcargen, and published in this Journal* some experiments upon the action of sulphuretted hydrogen upon nitrous ether, undertaken with the hope of obtaining the nitrogen compound corresponding to alcarsine. M. Laurent was, however, disposed to regard glycocoll as the amid of a bibasic acid $C_2H_4O_2$, the homologue of carbonic acid, and hence explained its monobasic acid character;† but to this view it is to be objected that the ordinary amids of bibasic acids are either neutral like oxamid, or acid without any basic characters like oxamic acid. Glycocoll is to be regarded as the isomere of glycollamic acid, precisely as the alkaloids, furfurine and benzoline are known to be isomeres—allotropic forms of the normal amids, and corresponds to ethammine less H_2+O_2, or to the product which should be obtained by the oxydation of SbEt. Its capacity to exchange H for K, is unlike that of acetic acid or alcohol, for the saline power of these belongs not to the carbohydrogen elements, but to the unreplaced H of the H_2O:—nor in this view is the saline hydrogen of glycocoll similar to that of oxamic acid; it is an atom of hydrogen in the ammonia itself, which is replaceable, as in asparagin, itself the binamid of a bibasic acid, and in paramid.‡

I conclude these observations by calling attention to the results obtained by Mr. Hofmann in decomposing the compound ammonias by a nitrite.§ I was the first to show that the elegant process by which Piria had succeeded in decomposing asparagin and

* Vol. iv, pp. 108—266, 350. † Ibid, vol. ix, p. 64.

‡ See my remarks on this subject in this Journal, vol. viii, p. 90.

§ Comptes Rendus des Travaux., Fev., 1851, p. 42, from Jour. de Pharmacie, t. xviii, p. 399.

some other amids, was applicable to the organic alkaloids, and that the action of nitric oxyd upon a dilute acid solution of nitrate of aniline yields nitrogen gas and phenol. Mr. Hofmann refers to my statement, but adds that in repeating my experiment the aniline was transformed into a brown mass containing a crystalline matter which was nitric phenol. He probably obtained the binitric species which is the first product of the action of nitric acid upon phenol, and which as described in my paper I actually prepared from the phenol thus obtained, by treating it with strong nitric acid, in the process for preparing the nitropicric acid. If, keeping in mind the great readiness with which phenol is attacked by nitric acid, he will take the trouble to repeat the experiment with a dilute solution of the salt, avoiding a large excess of nitric acid, he will not find it difficult to obtain the characteristic oily product which I have described, and which is not easily confounded with nitrophenesic acid.

By the use of nitrite of silver in accordance with my suggestion, for which he has found even nitrite of potash may be substituted, Mr. Hofmann was more successful. In distilling hydrochlorate of ethammine with a solution of nitrite of potash, nitrogen and nitrous ether were evolved, with a liquid containing apparently traces of alcohol and some drops of an oily matter. Similar results were obtained with butylamine, propylamine and amylamine. These nitrous ethers, as shown by M. Kopp and myself, are decomposed by sulphuretted hydrogen, the alcohols being regenerated.† In this way Mr. Hofmann succeeded in forming from the alkaloid, amylic alcohol. As the transformations of organic substances furnish us with the means of obtaining the corresponding bases, the problem of obtaining the alcohols of the propionic and butyric series is solved.

The reaction which gives rise to nitrous ether is not clearly explained; the nitrite obtained by double decomposition will be C_2H_7N, NHO_2, and may be resolved into $N_2+H_2O+C_2H_6O$. The simultaneous evolution of nitrogen gas and nitrous ether, or indeed the formation of the latter, can only be explained by some secondary action which it is not easy to foresee. We hope for more definite information upon the subject.

A curious subject of inquiry presents itself in regard to those bases which contain two and three equivalents of the alcoholic elements. If the decomposition were to take place in accordance with the formula above given, nitrate of biethylamine would yield $C_4H_{10}O$, which is the ether of alcohol, or its isomere butyric alcohol, and triethylamine, $C_6H_{14}O$, which is the formula of caproic alcohol. The decomposition of all the complex alkaloids by this reaction will be of great interest.

Montreal, C. E., Nov. 10th, 1851.

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* This Journal, vol. iv, p. 350.

ART. XVII.—*The Pendulum Experiment;* by Lieut. D. P. WOODBURY, U. S. Corps of Engineers.

WITH the exception of some unimportant allusions to the labors or suggestions of others the following article was drawn up in June last. It is an attempt to present the subject in a simple light, and to furnish a demonstration at once rigorous and easily intelligible. No allusion is made to those disturbing causes which we can either avoid or suppose to be avoided when the experiments are made in the most perfect manner.*

It is easy to understand the phenomena of the Foucault problem when the experiment is supposed to be tried at the pole of the earth or on the equator, but the case becomes more complex when the experiment is made at any intermediate point.

I will explain briefly the mechanical principles involved in this problem and then propose a new experiment analogous to that of the pendulum.

Let us suppose a piece of iron, or any heavy substance, which, for convenience, we will call a needle, suspended, in a horizontal position, in a north and south line, by a thread or wire. The needle will continue in the meridian for any period of time, and in so doing its prolongation must always pass through the same point on the prolongation of the axis of the earth. Around that point in each instant of time, it describes a small angle on the surface of the cone. The amount of that angular velocity, as compared with that of the earth during the same period, is easily calculated, either by comparison of the elementary arcs, or by the development of the cone and the comparison of the whole angular motion of the needle during a revolution of the earth with the corresponding motion of the earth or 360.° In either way we find

Angular motion of needle = angular motion of the earth × sine of latitude.

If the needle be suspended horizontally in any direction inclined to the meridian it will always maintain the same inclination and will therefore always maintain, on the surface of the cone, the angular velocity of the meridian as given above.

Now let us enquire under what influence or power the meridian needle moves, with uniform velocity, around the vertex of the cone. It is because every point of it has the motion of rotation round the axis of the earth due to its distance from that axis, and it moves as part of the earth. The *difference* between the velocity of the center and the velocity of the other parts is the cause of its angular motion on the surface of the cone.

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* For other articles in this Journal on this subject, see last volume, pp. 200, 251, 398.

A power applied to the center of gravity of any body has no effect on its angular velocity. If therefore in this rotation around the axis of the earth, the center of the needle were stopped while every other point of it retained a velocity equal to the difference between its former velocity and that of the center, the needle would continue to move, round a vertical line, with the same angular motion which it before maintained around the vertex of the cone.

Let us now suppose a second needle, which, during a revolution of the earth, has the same absolute velocity at every point, viz.: that due to its center. It was the difference in velocities that caused the angular movement of the first needle. But we have by supposition restored that difference. We have applied, as it were, to the first needle, in an opposite direction, the precise power which caused its own angular movement. So that if we take the first needle or meridian as the line of comparison, the second must continue to move from it with a uniform angular velocity equal to that of the first around the vertex of the cone.

But we may view the phenomena in another and perhaps more simple light.

The second needle, supposed to start on the meridian, with equal velocities at all points, will no longer continue on the meridian. The north end will continually make to the east of it, and the south end fall equally to the west of that line. The center is compelled to move on the parallel of latitude, and every point of the needle, having the same velocity, must move over the same distance, and this it can only do, when, in every instant of time, all the points describe equal and parallel lines—the rate of the motion being constant, viz.: that of the center—the direction constantly varying with that of the center. It follows that the needle must continue equidistant at all points from its first position supposing distances to be measured on the surface of the cone. Let us now, after the manner of Lieut. Hunt in his elegant geometrical solution of the problem published in the National Intelligencer, develop the cone on the tangent plane through one of its elements—say the one corresponding to the first position of the needle. All the positions of the needle become parallel—the several points continuing on equal though not concentric arcs: and at every point, the angle between the needle and the corresponding meridian line, is equal to the angle between the latter and the first meridian, or the sum of the angles described between the two around the vertex of the cone.

The pendulum, after a few oscillations, is precisely in the situation of the second needle. Whatever motion of rotation it may have, that motion is kept up equally on both sides of the central point, so that the ball may be regarded as having throughout the motion due to its central position alone. The motion of

its plane has therefore been accounted for. It is now evident that the Foucault problem admits of some extension.

1. Let us suppose a continuous hollow ring filled with water or some other fluid, the heavier the better, and suspended symmetrically in a vertical plane by a proper cord. Let the fluid be kept in continual circulation by means of a lamp attached to one side of the ring. Then will the ring revolve precisely like the plane of the pendulum and for the same reason:—because the suspended mass is compelled to be equally on both sides of the vertical axis or suspending cord. The rotation of the ring however must be somewhat retarded by the material of the ring, lamp, &c., which do not partake of the motion of the fluid.

2. Let a similar ring be placed symmetrically upon a small inflexible axis parallel to the axis of the earth, and let the fluid be kept in circulation as above. The ring will rotate no longer with the pendulum of our latitude, but with the pendulum at the pole of the earth, or once in twenty-four hours.

Again let the axis of the ring receive any direction, the velocity of rotation will be that of the pendulum at the place whose vertical line is parallel to that direction, or equally inclined with it to the axis of the earth.

3. Instead of a hollow ring let us suppose a solid ring or plate made to revolve round an axis at right angles to its plane by some attached machinery while the whole is supported symmetrically, by a vertical cord or axis, or by an axis inclined, as above, around which it is free to move. The result will be the same as in 1 and 2.

It would seem therefore that the Foucault experiment for all latitudes may be illustrated at any place, but it must be admitted that there are many practical difficulties to overcome—particularly when the principal axis is not vertical. These suggestions are thrown out for those who have the leisure, the means and the taste for such pursuits.

Some have supposed that any horizontal plate or bar, suspended by a cord, would continue to revolve like the plane of the pendulum if the proper motion could once be communicated —forgetting that the retarding force of friction, which is always acting, must soon destroy the motion communicated by an *impulse*. If all friction could be destroyed the bar would indeed maintain *any* rate of motion once communicated to it.

It has been suggested that the proper motion would be communicated to a suspended bar if its two ends were suddenly reversed and then allowed to move with the velocities previously acquired. The motion thus communicated would be *double* the one desired. The proper motion is communicated by the pendulum or by the means described above. To keep that motion up and overcome the friction always acting, the power must be continually renewed.

ART. XVIII.—*Peculiarities of the Climate, Flora, and Fauna of the South Shore of Lake Erie, in the vicinity of Cleveland, Ohio;* by J. P. KIRTLAND.

VERY erroneous opinions are entertained by even intelligent people, respecting this section of country, so far as its climate and the species of the animal and vegetable kingdoms are concerned. A series of observations, embracing a period of ten years, have disclosed some interesting facts upon these points.

The locality where these observations were made, is situated five miles west of Cleveland, half a mile from the lake, one hundred and fifty feet above its surface, and fully exposed to its influence. During the ten years, the temperature has in no instance fallen below zero; while at Columbus, Marietta and Cincinnati, situated from 120 to 150 miles to the south, it has frequently sunk to 5°, and has occasionally fallen to 10°, at some of those places. Their latitudes are as follows, to wit:

Point of observation near Cleveland,

North, ...	41° 31′
Columbus, ...	39° 57′
Marietta, ...	39°
Cincinnati, ...	39° 5′ 54″

The more tender vegetation is usually cut down in all northern Ohio—a few localities excepted—within five days of the 25th of September. The lake shore is an exception. Dahlias, maize, and sweet potatoes are generally killed simultaneously here, and at Cincinnati—never before the 25th of October, and sometimes not until late in November. In one instance, at least, the lake shore escaped two weeks later than did Cincinnati.

At the present moment, October 25, vegetation is as verdant and thrifty as it has been at any time during autumn, though it was cut down throughout the West generally several weeks since.

The foliage of the fruit and forest trees, having subserved its purposes, is falling without the intervention of frost, and the wood of the more tender trees, such as the peach and cherry, has attained a maturity that will render it sufficiently hardy to withstand the impressions of cold during winter. This occurring annually, gives to those trees a degree of vigor, health and productiveness not to be met with in localities where their growth is suddenly arrested by frost, at a period when they are immature.

In the middle and southern sections of Ohio, spring sets in during the month of March—perhaps earlier. The warm winds

blowing up the valleys of the Mississippi and Ohio, in conjunction with other causes, bring forth vegetation earlier; but cold weather and disastrous frosts too often follow.

While these changes are progressing in those parts of the state, winter will remain steadfast at this point. Little advancement will be made by spring, so long as any considerable bodies of ice float upon the lake, even as low down as Buffalo. No sooner do they disappear than spring sets in with a reality, and vegetation puts forth with *sub-arctic* rapidity.

The lake rapidly imbibing heat at this season, becomes a safeguard against any subsequent vernal frost. Its influence was manifested in a satisfactory manner, early in the present season. On the 1st of May, spring seemed to be fully established; fruit trees had blossomed, and in some localities young fruits had formed. The morning was cold and the temperature declined during the day and evening. At 2 o'clock, P. M., it was 48° Fahrenheit; at 7, 34°; and at 9, 32°. The atmosphere was calm and clear, indicating to an inexperienced observer the approach of a destructive frost. At 10 o'clock, P. M., it had risen to 40°; a heavy cloud of haze hung about twenty degrees above the lake, and soon overspread the whole horizon. The morning of the following day was warm and misty; by 12 o'clock, A. M., it was clear and spring-like. Not a fruit-germ was injured on the lake shore. A different state of things occurred throughout the West and Southwest, where no local influences interposed. The temperature steadily declined, without intermission, during the day and night, down to about 26°. The day following was cold and blighting, and fruits were generally destroyed.

The modes by which the lake exerts its influence on such occasions do not appear to be uniformly the same at different times.

On the approach of a cold night, as in the instance above noticed, the warm emanations condensing may give off caloric, and obscure the atmosphere with haze, mist, or clouds, when no frost will occur.

Under circumstances apparently similar, on the approach of a cold night, neither haze mist nor clouds may form, but a stiff breeze springs up, and the stars become unusually brilliant. The thermometer vacillates between 32° and 38°, rising with the gusts of wind, and falling during the intervals of calm. *Then* no frost will appear.

Again, none of those modifying causes may intervene, but the temperature may fall below freezing point, ice form on the surface of water, and the expanded fruit, leaves and blossoms congeal. Under such circumstances, the first rays of the rising sun, the next morning, will be arrested by a haze, which will soon thicken, and before noon a warm rain will probably fall. The

frost will be abstracted so gradually from frozen vegetation as not to impair its vitality.

These contingencies have all occurred within the period of our observations. The year 1834 proved an exception.—The general cold prevailed over the local warmth of the lake; freezing weather continued two or three days, and fruits were cut off, even on the shore of the lake.

In autumn, this great body of water begins to part with its warmth to the colder incumbent atmosphere, and the process continues during the winter. While its progress is most rapid, strong southerly winds prevail at the earth's surface, while volumes of clouds, at a high elevation, may at the same time be moving rapidly in an opposite direction.

These counter-currents have sometimes given origin to a phenomenon in the city of Cleveland, not well understood by all of its good citizens. The vane of the lofty spire of the Baptist church, standing on a high ridge of ground, may point steadily to the north, while that on the low cupola of the First Presbyterian church, situated on a less elevated plateau, may be directed to an opposite point of compass, with a stiff southerly breeze at the same time.

Cool north winds begin to prevail about the middle of October. The emanations from the lake then begin to condense and pass off to the south, in the form of thick clouds, without discharging, at first, much rain. About the 20th of October the cold from the north seems to gain the ascendancy; squalls of rain, hail, and rounded snow appear alternately, with intervals of clear and warm weather. These squalls always precede the autumnal frosts. Our gardeners feel no apprehension for their tender vegetables till these premonitions have appeared.

Common observations, as well as the more sure test, the rain-guage, show that larger amounts of vapor from the lake are carried south, condensed in the form of rain and snow, than fall in this vicinity.

During winter comparatively little snow falls, and still less accumulates here, though it may be abundant on the higher grounds, thirty or forty miles in the interior.

This region is also not so frequently favored with showers in summer as the central portion of the state. Long and severe droughts often prevail, but they are in part counteracted by moisture in the atmosphere. This quality sustains vegetation, and also imparts a blandness and freshness to the atmosphere during the hottest days of summer, very observable on approaching the lake from the interior. During that season it is peculiarly pleasant and invigorating to invalids, and equally harrassing to them during the spring season.

The indigenous vegetation of this vicinity is of rather a southern type—shown by the absence, in a great measure, of evergreens, and the occurrence of more southern genera, as the Cercis, Ilex, Æsculus, Nelumbium, Gleditschia, Magnolia, &c. Eliott's Botany of South Carolina and Georgia has been found to be a convenient hand-book for investigating our flora. On the other hand, strange hyperborean plants are frequently found, which have been washed down from the far Northwest, through the chain of great lakes.

Many of our birds are species whose most northern ranges of migration have been assigned many degrees south of this, by ornithologists. The hooded, Kentucky, yellow-throated-wood, cœrulean, and prairie warblers, annually rear their young in this vicinity. Trail's fly-catcher, and the piping plover, have been repeatedly seen here, and the purple ibis is an occasional visitor. The list might be greatly extended.

Great numbers of the Sylvicolæ semi-annually congregate here, during their migrations, and seem to make it a resting-place, both before and after passing the lake. More northern species occasionally resort here during winter, for the purpose of obtaining food, or are driven here by storms: such are the pine-grosbeak and the white owl. The Bohemian wax-wing visits us almost every winter, and sometimes in large flocks. The pinefinch is described, by some ornithologists, as resorting to the United States only at long intervals, and during winter. It visits our gardens and grounds in numerous flocks, every season, early in July, and remains here till the ensuing spring.—The young, at their first appearance, still retain much down about their plumage, and cannot have been long absent from their nests. The food of these birds is Aphides during summer, and at other times small seeds of grapes, and other vegetables.

The insect tribes show still more strikingly southern affinities. The Papilio Cresphontes, figured and described by Boisduval and Le Conte, as the *Papilio Thoas*, has been repeatedly taken here; though it has been considered as exclusively southern in its resorts. In the South, the larva feeds on the orange and lemon—here, Major Le Conte informs me, it lives on the Hercules-club.

The Papilio Ajax and P. Marcellus have also been described as southern insects; and the late Mr. Doubleday located the former exclusively in Florida, and fixed the most northern limit of the latter in Virginia. Still they are common at this point, and subsist in the larva state, on the pawpaw. An undescribed species of Libythea has been taken in Northern Ohio; it has been found, also, in South Carolina, and is without doubt legitimately a Southern species.*

* See the figure and description in No. 76 of the Family Visitor.

The Chœrocampa tersa, an elegant miller, was taken in our garden, in the month of May last. Dr. Harris describes it as a native of South Carolina, where it feeds on a species of plant which does not grow at the North.* The food it finds as a substitute, has not been ascertained.

ART. XIX.—*Analysis of a Magnetic Iron Pyrites, containing Nickel, from Gap Mine, Lancaster Co., Pennsylvania;* by M. H. BOYE, Professor of Chemistry and Natural Philosophy, Central High School, Philadelphia.

DESCRIPTION.—Color, dark gray drawing into bronze; lustre metallic; texture, granular interspersed in some places with copper pyrites, hornblende and black mica. Powder dark greenish; streak black. Attracted by the magnet, and possessing distinct magnetic polarity. Specific gravity of the whole piece (including interspersed gangue,) 4·193. Somewhat porous and easily broken.

1. Five grammes of finely pulverized ore were digested with strong nitric acid with addition of chlorohydric until the separated sulphur had assumed a perfectly yellow color. It was then diluted with some water and filtered. The residue was washed, dried and incinerated, yielding, after deduction of the ashes of the filter, *silica and insoluble silicates* 1·273 grammes, or 25·46 *per cent.*

2. The filtered solution from the silica (§1) was precipitated in cold by carbonate of lime (whiting) previously triturated with water, till after standing for twenty-four hours it showed still a film of fine undecomposed carbonate of lime floating on the top of it. The precipitate was then separated by filtration and washed moderately with water. The filtered solution, after being acidulated, yielded no precipitate by sulphohydric acid.

3. By the addition of ammonia in slight excess to the above, it yielded a black precipitate of sulphuret of nickel. The passage of sulphohydric acid was then continued through it for some time, by which the sulphuret of nickel settled to the bottom, and the supernatant solution became clear, and of a light yellow color, indicating no nickel in solution. The sulphuret of nickel was then filtered and washed; the filtered solution being rejected for further use.

4. The precipitate obtained by carbonate of lime (§ 2) was redissolved in chlorohydric acid, filtered from the separated silica

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* See Dr. Harris's very valuable Catalogue of American Sphinges, in vol. xxxvi, Amer. Journal of Science and Arts. The student of Entomology will there find the history of this intricate family made plain by the labors of Dr. H.

(from the whiting) and sulphohydric acid passed through it to saturation. The precipitate, containing much free sulphur, was filtered, washed and digested with sulphohydrate of ammonium. A black precipitate remained, which after washing was oxydized by nitric acid, filtered and precipitated by hydrate of potassa after addition of some ammonia, and thus after filtering, washing, drying and incinerating, yielded oxyd of copper 0·075 grammes, or 1·50 per cent., equivalent to 1·197 *per cent. metallic copper.*

5. The filtered potassic solution from the oxyd of copper (§ 4) became dark by the addition of a few drops of sulphohydrate of ammonia and deposited a black precipitate, which, collected in a counterpoised filter and dried in water-bath, weighed 0·019 grammes or 0·38 per cent. This, dissolved in diluted nitric acid, yieded by addition of diluted sulphuric acid an abundant precipitate of sulphate of lead. The filtered solution from this yielded, by a current of sulphohydric acid, an inconsiderable amount of sulphuret of copper amounting to 0·003 grammes or 0·06 per cent., equivalent to 0·04 *per cent. metallic copper.* The above 0·38 per cent. contained, therefore, after deduction of the sulphuret of copper 0·32 per cent. sulphuret of lead, equivalent to 0·27 *per cent. metallic lead.*

6. The filtered solution obtained from the above sulphurets by digestion with sulphohydrate of ammonium (§ 4) was acidulated with chlorohydric acid and yielded an abundant precipitate of the usual appearance of precipitated sulphur which was filtered and washed. It contained neither antimony nor arsenic but left by gentle heating 0·005 grammes or 0·1 per cent. of sulphuret of copper, equivalent to 0·066 *per cent. metallic copper.*

7. The main solution from § 4, after the passage of sulphohydric acid through it and filtration of the separated sulphurets, was evaporated to a smaller volume and then boiled with the addition of nitric acid, for the conversion of monoxyd of iron into sesquioxyd. The latter was then again precipitated as in the first instance by carbonate of lime, filtered and washed. The filtered solution was mixed with ammonia in very slight excess and sulphohydric acid passed through it until it settled clear. It thus yielded a second precipitate of sulphuret of nickel which was separated by filtration, washed and added to that of § 3.

8. The last filtered solution (§ 7) from the sulphuret of nickel was of a clear but strong yellow color. By standing for several days it deposited a precipitate of sulphuret of nickel, was filtered, washed and strongly incinerated, yielding 0·002 grammes or 0·04 per cent. which calculated as oxyd is equivalent to 0·032 *per cent. metallic nickel.*

9. The sulphurets of nickel (§ 3 and 7) were oxydized by nitric acid with the addition of chlorohydric, till the sulphur became perfectly yellow. The solution was filtered and mixed

with ammonia in great excess, by which a small portion of sesquioxyd of iron precipitated, which was separated by filtration. The filtered ammoniacal solution was then mixed in a capsule with carbonate of soda in excess, evaporated to dryness and heated till it changed its color. It was then re-dissolved in boiling water and filtered. The filtered solution contained no trace of nickel. The precipitate was washed, dried, incinerated and weighed, yielding monoxyd of nickel 0·287 grammes or 5·74 per cent. equivalent to 4·517 *per cent. metallic nickel.* The oxyd of nickel contained neither cobalt nor manganese.

10. To estimate the amount of iron, one gramme of the ore was digested as before in nitro-muriatic acid. The filtered solution was precipitated with ammonia in great excess. The precipitated sesquioxyd of iron was filtered and washed with ammonia and then digested with hydrate of potassa, again filtered, washed, re-dissolved in chlorohydric acid and precipitated by ammonia, filtered, washed, dried and incinerated, yielding 0·618 or 61·3 per cent., which after deduction of the contained oxyd of nickel (see § 16) leaves 59·62 equivalent to 41·34 metallic iron.

11. The potassic solution from the digestion of the sesquioxyd of iron was super-saturated with nitric acid and precipitated by ammonia by which it yielded a precipitate of alumina amounting to 0·017 grammes or 1·70 per cent.

12. The solution from § 10 was acidulated by chlorohydric acid, and then sulphohydric acid passed through it until saturated. The precipitate was separated by filtration, the filtered solution slightly super-saturated with ammonia, and sulpho-hydric acid passed through it until the sulphuret of nickel settled clear. The latter was filtered, dissolved in nitro-muriatic acid, filtered, evaporated to dryness with carbonate of soda in excess, heated, re-dissolved in boiling water, filtered, washed and incinerated, and yielded 0·041 grammes or 4·1 per cent. oxyd of nickel, equivalent to 3·15 *per cent. metallic nickel.* The filtered solution contained no nickel.

13. The solution filtered from the sulphuret of nickel was barely neutralized by chlorohydric acid and evaporated at a low temperature to a smaller volume. The separated sulphur had a dark grayish color and was therefore separated by filtration and incinerated with the sulphur from the oxydation of the sulphuret of nickel (§ 12) but amounted to no appreciable quantity.

14. The filtered solution (§ 13) indicated by the addition of ammonia and oxalate of ammonia only a trace of lime. After filtration the addition of phosphate of soda caused no precipitate.

15. To determine the quantity of sulphur, one gramme of the finely pulverized ore was mixed with five grammes of nitre and the same amount of carbonate of soda and heated to beginning fusion in a platinum crucible. The mass was digested with

water and filtered. The solution was super-saturated with nitric acid and precipitated lukewarm by chlorid of barium. The sulphate of baryta was filtered, washed with boiling water, dried and incinerated, weighing 1·800 grammes, equivalent to 24·84 per cent. of sulphur.

16. Deducting the difference of oxyd of nickel in the two experiments (§ 8, 9 and 12) from the amount of oxyd of iron (§ 10) the analysis will stand thus:

	In 100 parts.
Iron,	41·34
Nickel,	4·55
Copper,	1·30
Lead,	0·27
Sulphur,	24·84
Silica and insol. silicates,	25·46
Alumina,	1·70
	99·46

17. After deducting the sulphur necessary for the conversion of the lead (0·04) and the nickel (2·47) into sulphurets, and the sulphur (1·32) and iron (1·15) necessary to form with the copper, copper pyrites, there remains 21·01 sulphur, for 40·19 iron. The latter would require for its conversion into monosulphuret, 23·25 sulphur. A small portion of the iron may however be due to the gangue.

ART. XX.—*On the "Clinochlore" of Chester Co., Pa.;* by Mr. W. J. CRAW, First Assistant in the Yale Analytical Laboratory.

THE optical and blowpipe characters of this mineral were described by Mr. Wm. P. Blake in the American Journal of Science, 2nd Series, vol. xii, p. 339; also vol. xiii, p. 116.

The remarkable results which he obtained by examination with polarized light have led to my undertaking a complete analysis of the mineral, in order to ascertain whether it differed or not in chemical constitution from chlorite. Qualitative analysis gave as the constituents, Si, Al, Fe, Cr, Mg, and H with a trace of Ca. Protoxyd of iron was tested for, but none could be detected.

Two analyses of the mineral were made. In the first, it was fused with carbonate of soda, dissolved in dilute nitric acid, and evaporated to dryness to separate silica in the usual manner. To the solution in hydrochloric acid filtered from the silica, alcohol was added, and the whole heated for some time in order to reduce the chromic acid to the state of oxyd. The alumina, peroxyd of iron, and oxyd of chromium were then precipitated together by carbonate of baryta, redissolved in hydrochloric acid, and,

after removal of the baryta by sulphuric acid, again precipitated by ammonia. The mixed oxyds were ignited, weighed, and fused with carbonate of soda and nitre. From the solution in nitric acid the iron and alumina were precipitated by ammonia, and afterwards separated by caustic potash; the chromium was determined in the filtrate.

From the solution filtered from the precipitate by carbonate of baryta, the dissolved baryta was removed by addition of sulphuric acid, and the magnesia then precipitated by phosphate of soda. The second analysis was conducted in a manner similar to the first, except that the iron, alumina, and chromium were weighed together, and not separated from each other. Two determinations were made of the water, giving, respectively, 12·631 and 12·567 per cent.; mean = 12·599.

The results were the following; in calculating the proportion of oxygen from the second analysis, the amount of iron and chromium found in the other analysis was deducted from the joint amount of the peroxyds, and the remainder assumed to be alumina.

	I.	II.	Oxygen in I.		In II.	Mean.
$\ddot{S}i$	31·344	31·78	16·284		16·513	16·40
$\ddot{A}l$	17·467		8·164	8·025		
$\ddot{F}e$	3·855	22·71	1·156	9·848–1·156	9·709	9·78
$\ddot{C}r$	1·686		0·528	0·528		
$\dot{M}g$	33·44	33·64	13·376		13·456	13·42
$\dot{H}$	12·599	12·599	11·19		11·19	11·19
	100·391	100·729				

Ratio between the oxygen of the silica, peroxyds, magnesia and water = 1·677 : 1 : 1·372 : 1·144 or 15 : 9 : 12 : 10 nearly. This ratio is the same as that calculated by Rammelsberg, (2d Supplement, p. 36,) from the mean of seven analyses of chlorite. He changes the proportion to 15 : 9 : 12 : 9 on the ground that hygroscopic moisture is always contained in the analyzed mineral.

This would give as the formula either,

$$\dot{M}g^6\,\ddot{S}i^5+3\dot{H}\,\ddot{S}i+9\dot{M}g\,\dot{H},$$

or $$2\dot{M}g^6\,\ddot{S}i+3\dot{H}\,\ddot{S}i+3\dot{M}g^2\,\dot{H}^6,$$

to the latter of which Rammelsberg gives the preference.

According to Gerhardt's theory, who considers the protoxyds, peroxyds and water as replacing one another, this will come under that class of minerals, in which the oxygen of the silica is to that of the other constituents in the proportion of 1 to 2. Chemically considered, the only difference between the Clinochlore examined and the ordinary chlorite appears to consist in the fact that the former contains chromium. It is, however, as has been shown by Mr. Blake, widely different in its optical properties.

ART. XXI.—*Phosphoric Acid in Normal Human Urine;* by Dr. D. BREED.

NOTWITHSTANDING the importance of all knowledge appertaining to physiology and pathology, neither chemist nor physician has heretofore made any considerable research in relation to phosphoric acid in urine. But the accurate and expeditious method for the determination of phosphoric acid, proposed by Professor Liebig, forbids our remaining longer in ignorance of facts which may be of great value in the treatment of disease. This method consists simply in the titrition of urine, with a solution of the perchlorid of iron, until the filtrate from the mixture gives the well known blue reaction with ferrocyanid of potassium. It is based upon the fact that either a neutral or acetic acid solution of phosphoric acid gives with perchlorid of iron an insoluble precipitate, whilst the peroxyd of iron is readily dissolved by acetic acid. If a solution of phosphoric acid containing acetate of soda be treated with perchlorid of iron, we have the following reaction—$PO_5 + 3(Na\,O, \overline{A}) + Fe_2Cl_3 = Fe_2O_3, PO_5 + 3(Na\,Cl) + 3\overline{A}$.

The solution of perchlorid of iron is most conveniently made by dissolving in nitro-hydrochloric acid 15·656 grammes of iron, and evaporating to dryness, to expel the excess of acid, then adding nitric acid, and again evaporating to prevent the existence of protochlorid, and afterward, re-dissolving the product in 2000 cc. of water. Every cc. of this solution will precipitate ten milligrammes of phosphoric acid. Instead of the above, a solution of the perchlorid of iron of unknown strength may be used, and the iron be determined. Or, the strength of the solution of iron may be found by titrition with a solution of phosphoric acid of known strength. In all the above methods the solution of iron must be free from proto-chlorid.

If the urine, of which we would determine the phosphoric acid, has become alkaline by the decomposition of urea, some of the phosphoric acid may have been precipitated with lime or magnesia, and it will be necessary to dissolve the precipitate by a few drops of hydrochloric acid. The urine is measured and shaken well; with a pipette 100 cc. or more are drawn off into a beaker glass; acetate of soda (much if hydrochloric acid has been used) and free acetic acid are added. The urine is then treated with a solution of perchlorid of iron from a burette,—frequently testing, until the phosphoric acid is saturated, and we have a drop of iron in excess. To test for iron in excess, lay a filter on a white porcelain plate, or on a glass supported by white paper, and moisten it slightly with ferrocyanid of potassium; with a glass rod bearing a drop of urine, press a double filter upon the paper containing the ferrocyanid of potassium, and if there be an excess of iron, in three or four seconds we have the blue

reaction: noting the quantity of the solution of perchlorid of iron used, we proceed in like manner with two other portions of urine; if the results agree, we then calculate the amount of the solution of iron required for all the urine from what was found necessary in 100 cc. and multiplying the number of cc. required in all, by the amount of phosphoric acid known to correspond to one cc. of the iron, we have the amount of phosphoric acid in the whole urine. By this method, a physician who has but little leisure, may make each day many determinations of phosphoric acid.

The amount of phosphoric acid from an individual in health, and partaking of a uniform diet, is nearly uniform; but experiments now being made, show that diet, disease, and medicines vary the amount of phosphoric acid from the normal standard. These considerations, whilst they show the necessity of a more extensive and varied course of experiments, indicate that farther research may lead to discoveries in regard to the constituents of urine which may be of the highest importance in therapeutics. Prout, Bright and others have but opened a field of labor and discovery upon which the chemist is earnestly called to enter. Numerous statistics in regard to normal urine from individuals of different temperaments and modes of living, are first necessary. Pathologists must critically observe the effects of various maladies upon the urine,* and then we shall have the data whence to reason, and possibly to discover, not only the causes and cure of calculus without the use of the surgeon's knife, but light also may be thrown upon the treatment of many other diseases which now baffle the skill of the physician.

Determinations of Phosphoric acid in Urine.

The following results were obtained from the urine of four persons, mostly from that of one person.

Urine of 24 hours.	Phosphoric acid.	Urine of 24 hours.	Phosphoric acid.
1645 cc.	2·118 grms.	1607 cc.	3·931 grms.
1150	2·909	2075	4·336
1690	3·454	1208	2·719
1675	2·611	1640	2·837
1462	3·647	2655	6·051
1462	3·647	1777	5·126
†1707	3·744	2058	3·888
†1707	3·744	1768	3·407
†1701	6·447	985	3·384
†1701	6·447	1561	3·941
†1380	2·862	1916	4·946
†1380	2·862	740	2·523
Average	{ 1000 cc. gives 2·317 grms. phosphoric acid.		
	{ 24 hours gives 1610 cc. urine, and 3·732 grms. phosphoric acid.		

* Dr. Breed has discovered phosphoric acid in a fluid which Professor Vogel of Giessen obtained by the operation of paracentisis abdominis. From 3105 cc. of the fluid, 4,648 grammes of phosphoric acid were obtained. In the urine of this patient during twenty-four hours, only 1·2 grammes of phosphoric acid were found.

† Average of two days.

Results from a person who drank an excess of three pints of water daily.

Urine of 24 hours.	Phosphoric acid.
2470 cc.	4·288 grms.
2407	4·274
1548	4·006
1919	4·344

Average { In 1000 cc. of urine, 2·027 grms. phosphoric acid.
" 24 hours 2086 cc. urine and 4·228 " "

Results from a person who drank but half his usual amount of fluid.

Urine of 24 hours.	Phosphoric acid.
787	3·807
1220	4·218
950	3·904
997	4·133

Average { In 1000 cc. of urine, 4·062 grms. phosphoric acid.
" 24 hours 988 " 4·015 " " "

Results from urine secreted during the waking hours, and during those of sleep.

Night.		*Day.*	
Urine.	Phosphoric acid.	Urine.	Phosphoric acid.
627 cc.	1·472 grms.	835	2·174
627	1·472	835	2·174
650	1·169	1057	2·575
650	1·169	1058	2·576
671	2·385	1030	4·061
671	2·385	1030	4·062
635	·999	745	1·904
635	·999	745	1·904
980	1·611	1078	2·277
540	1·033	1236	2·374
360	1·258	625	2·127
450	1·223	765	2·367
325	·634	1103	2·718
325	·634	1454	4·019
462	·927	555	1·796
185	·528		
415	1·364		
Av'age	{ In 1000 cc. urine, 2·763 grms. phos. acid. " 24 hours 1748 cc. urine & 4·831 phos. acid.		

Av'age { In 1000 cc. urine, 2·284 grms. phosphoric acid.
" 24 hours 1854 cc. urine, and 4·234 grms. phosphoric acid.

Results from a person who drank an excess of water.

Night.		*Day.*	
Urine.	Phosphoric acid.	Urine.	Phosphoric acid.
1050	1·351	1420	2·936
910	1·496	1497	2·781
230	·827	1318	3·179
435	·934	1484	3·410
Av.–In 1000 cc. urine, 1·755 grms. phos. acid.		Av.–In 1000 cc. urine, 2·130 phos. acid.	

Results from a person who drank very little.

Night.		*Day.*	
Urine.	Phosphoric acid.	Urine.	Phosphoric acid.
245	1·132	542	2·674
377	1·549	843	2·668
435	·983	...	....
382	1·516	615	2·617
Av.–In 1000 cc. of urine, 3·599 grms. phos. acid.		Av.–In 1000 cc. urine, 3·979 phos. acid.	

Results from urine secreted before and after dinner, exclusive of the hours of sleep.

Before dinner.		*After dinner.*	
Urine.	Phosphoric acid.	Urine.	Phosphoric acid.
360	·995	260	·908
360	·995	260	·908
815	1·377	263	·901
919	1·510	317	·863
405	1·415	220	·712
585	1·595	180	·772
803	1·485	300	1·233
612	1·510	492	2·174
613	1·510	310	1·274
962	1·845	Av'age { 1000 cc. urine, 3·745 grammes phos. acid.	
245	·722	{ 24 hours 1120 cc. urine, & 4·881 grms. " "	

Av'age { 1000 cc. urine, 2·239 grammes phos. acid.
{ 24 hours 1418 cc. urine, 3·177 grammes phos. acid.

Results from a person who drank excess of water.

Before dinner.		*After dinner.*	
Urine.	Phosphoric acid.	Urine.	Phosphoric acid.
960	1·447	460	1·490
1212	1·826	280	·955
363	1·164	955	2·011
1004	1·733	480	1·678
Av'age { 1000 cc. urine, 1·743 grms. phosphoric acid.		Av'age { 1000 cc. of urine, 2·820 grms. of phosphoric acid.	

Results from a person who drank very little.

Before dinner.		*After dinner.*	
Urine.	Phosphoric acid.	Urine.	Phosphoric acid.
360	1·627	182	1·047
620	1·751	223	·916
560	1·473	385	1·725
230	·891		
Av'age { 1000 cc. of urine, 3·244 grms. phosphoric acid.		Av'age { 1000 cc. urine, 4·541 grms. of phosphoric acid.	

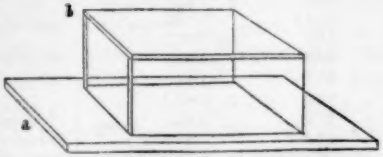
Art. XXII.—*Instructions for making wet Preparations of Animal Substances;* by Henry Goadby, M.D., F.L.S., formerly Dissector of Minute Anatomy to the Royal College of Surgeons of England.

There are many preparations of entire animals no less than dissected portions of them, which can be well displayed only in vessels with flat surfaces, in contradistinction to round or oval bottles; but, from their greater size, they cannot be displayed in vessels constructed on the principle of those already described. For all such, I build up a *box* of glass, consisting of four sides, the bottom plate (or slide) and the top, or cover.

These vessels are confessedly difficult to make; yet they form the most attractive and beautiful exhibitions that can be put into a museum. The trouble I have had with these upright vessels, no less than my great desire to submit them and the preserving fluids, to the only satisfactory test—time, has retarded the publication, on my part, of the several processes herein referred to.

Having settled the length, depth, and width for an upright box, the glass for the sides should be selected of sufficient substance for the bulk and weight of fluid the vessel is destined to contain; and it will frequently happen that the ends (by which I mean the two lengths of *least diameter*, calling the larger and outer portions the sides, as in the annexed figure, 9,) require to be somewhat thicker than the sides to insure sufficient surface for a joint. The glass should be cut as true and square as possible in the first instance; the two side pieces and the two end pieces should be connected together, respectively, with the marine glue, forming two pairs of glasses. First, bevel all the outer edges, of all the glasses, on the metal plate, as before directed; and then proceed to make perfectly flat the extreme ends of each pair of glasses; this, the most important, is, at the same time the most difficult part of the work, and such as can be accomplished only by practice; the position of the glass in the hand must be frequently changed, for the pressure of two fingers on one side, opposed to the thumb on the other, will have a tendency to incline the glass to an angle of 45°, whilst the operator believes he is holding it perfectly upright. It frequently saves time to grind till a smooth but inclined surface extend from one outer edge of the pair of glasses to the other, and then change its position in the hand—the probability is, that there will be the like tendency to form a similar angle, although reversed, and by carefully watching and measuring, the operation may be suspended at the point where perfect flatness obtains, and just before the inclined plane can be formed in the other direction. A small brass square will be found of considerable importance in testing the truth of the grinding, but the most severe test is that which I always resort to, namely, to wet the ground surface of glass as lightly as possible and place on it a plate of plate-glass—the sides of which, for all practical purposes, are parallel: if true and flat, the plate-glass will be seen to touch every part of the ground surface and form with it a ⊤. Now, wipe all the glass just tested quite dry; *breathe* upon the ground surface, and quickly apply the plate-glass—if pure, the moisture of the breath will be equally diffused along its surface, and the contact be so perfect that the ground surface will hang suspended for several seconds from the plate-glass. If the work endure this test, there need be no doubt about making a permanent joint.

9.



Again use the iron-plate, un-cement and clean the glasses, prepare the ground surfaces of the ends and the flat surfaces of the sides against which the ends are to abut, with the naphtha solution; return all the glass to the iron-plate (if small enough to lie

there at the same time) and place marine glue on the painted flat surfaces of the sides; when melted put on the *ends*, which will form three sides of an open square, then quickly place the other side on the ends, and carefully remove it from the plate to a piece of wood, or paper—the former, provided with a straight edge (like the cutting board) is the best. While the glass remains hot enough to keep the glue soft, press together and critically adjust the glued surfaces, taking especial care that the sides coincide with the angles of the brass square; it is most important to remove from the glue in the joints any extraneous particles of dirt. These preliminaries settled, the glass cold, and the glue hard, the operator will have four sides of a box—like a brick-maker's mould—without top or bottom; this he may now proceed to grind upon the metal plate by a circular motion—constantly turning the box in his hand to prepare it for the bottom: after this is accomplished, it must pass the ordeal of the former test; next the slide or bottom plate of glass must be heated on the iron plate, (it having been previously prepared with the naphtha solution which must also be applied to the ground surface of the box;) and then after melting the marine glue on the upper surface near the edge for the adhesion of the lower edges of the hollow box, this box is to be applied to the slide and the whole suffered to remain on the heated plate of iron long enough for the lower portion of the sides of the box to become sufficiently hot to form a joint with the slide, but without melting the joints previously made; when cold, the upper surface may be ground in the same manner as the bottom, and with like care, and when finished, the vessel will appear like figure 9.

To give additional support and resistance to the joints, Mr. Dennis, suggested to me the application of triangular bars of glass, which he called "angle pieces" cemented into the corners. In my experience of them they fail, for two reasons—one, because they cannot be ground with sufficient accuracy, and the other, that in cementing them the heat is generally so great as to decompose the glue. I have substituted the following plan with more success: I pour melted glue into the corners, and make the angles of the vessel hot enough to keep the glue fluid, while I cause it to run equally from the bottom to the top of the box—this plan has not disappointed my expectation of it in any instance.

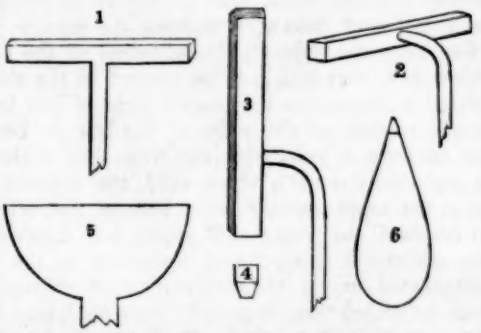
10.

I have made several upright vessels some of them of great dimensions (fifteen inches high, six inches wide, and four inches deep) and extremely elegant in their appearance. A preparation of Physalia pelagica (Portuguese

man of war), fig. 10, will give an idea of this form of vessel; the original box is eight inches high by two inches wide, five-eighths deep from back to front: *a* represents the front side, *b b*, the end pieces, *c*, a block of polished plate-glass half an inch thick to which the upright box is cemented, *d*, a thinner plate of glass forming with *c* a handsome pedestal and heavy support for the upright vessel.*

The joints of the pedestal, and of the box to the thick upper plate, must be made with Canada balsam or the *chloroform* preparation of marine glue for the sake of transparency; the box must be made, as before directed, with the patent marine glue. When large surfaces of glass are to be cemented together, the iron-plate is insufficient for the purpose and another plan must be had recourse to. I have already remarked that a red-hot soldering iron may be applied to the edges of glass with considerable impunity, and I avail myself of this fact in the manufacture of large upright vessels. Fig. 11 represents the several forms of such irons

11.

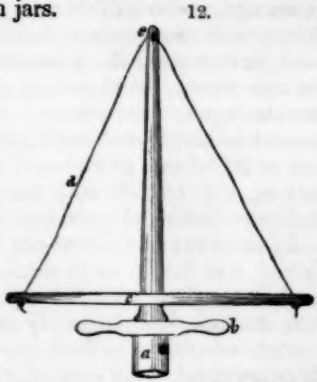


as I have found most desirable. Numbers 1, 2, 3 and 6, are reduced one-half in size; and 5 is more reduced; 1 and 2 are made of iron rod $\frac{5}{16}$ths inch square, welded to a round iron wire which is inserted into a wooden handle; 2 only differs from 1 in having the iron wire bent, as shewn in the figure—they are both eleven inches long, inclusive; 3 is intended to apply the marine glue to the inner angles of the boxes and upright vessels; an end view is shewn at 4. The handle of this last is differently fixed, and its length, including the wooden handle, is fifteen inches; 5 represents a much heavier tool and is designed to retain the heat for a longer time than either of the former. With a pair of such instruments large surfaces of glass can be well and expeditiously soldered.

* The mode of grinding a box of the height here mentioned, does not in any respect differ from that which has been described. Here, the end pieces are considerably elongated and must firstly be made true in the direction of their longer axis —the top and bottom is not to be ground until the hollow box be perfect.

It has sometimes happened in boxes and upright vessels, that in time the ends have been pressed in by the pressure of the atmosphere: to prevent this contingency I soldered on to the sides at the top and bottom, a bar of glass represented by the dotted lines *e*, *f*, fig. 10—they are not shewn at the bottom. These bars should be affixed to the sides before the vessel be made; they will thus, if exact in their length, serve as a guide to the true position of the ends, and being ground with the top and bottom of the vessel when in the form of a hollow box, will give increased surface for the joints to be made at these places respectively. The bars at the top are very useful as a means of fixing the threads by which the preparation is to be suspended; for this especial purpose I make four loops of the silk to be used (China three twist) and place two on each side of the vessel in the exact place they should occupy, and cement them firmly to the vessel by means of the bars—in mounting the preparation, I have merely to tie the threads which pass through it to these loops. In large vessels constructed on the principle lastly described, it is desirable to seal down the top glass with marine glue instead of the black cement. In this case the top glass should be of the exact size of the outside measure of the box and two small holes should be drilled in it at opposite corners—one to let the air out while the vessel is being filled, by means of the other. The process is similar, in all respects, to the description given in relation to preparation jars.

12.



To drill the holes, I use what is called in mechanics a "bob-drill," of which a figure is given. (Fig. 12.) The actual drill consists of a diamond, for a very small hole, a garnet, a crystal of corundum, or a fragment of the corundum, which may be obtained of emery merchants under the name of "emery stone." The operator will mount these drills for himself in a piece of thin tin rolled into a tubular form, with one end adapted to the drill stock.*

The part of the glass to be drilled should be marked either with a scratch diamond or the point of a file. It is only neces-

* The "bob" drill: *a*, the steel stock, 10¼ inches long perforated at *e* with a round eye for the transmission of the cord.—*b*, the circular weight either of iron or lead.—*c*, a turned handle 9¼ long, perforated with a hole in the center ⅜ths diameter, through which the drill stock can freely move, and at each end a hole for the cat-gut, *d*, to pass through and be tied in a knot. The drill is inserted at the bottom of the stock below *a*. A thong of dried eel-skin is a far stronger and much more durable cord for such a drill than cat-gut.

sary to use sweet oil, and any of the above named substances (I use all of them) will speedily cut a hole through the glass. Superfine emery in addition to oil may be used, but I believe that it cuts the drill without expediting the process.

For large vessels, I have been in the habit of giving increased strength to all the joints by covering them on the outside with a frame of glass consisting of strips about one-fourth of an inch wide; and, as the top glass is included in this arrangement, the corks plugged into the drilled holes are thereby kept in their place and concealed.

Lastly, I have made such preparations ornamental by water gilding the outer frame or moulding. This last process tries the joints severely; for the gilder's cement which must be applied to the moulding, contracts as it dries, and when dry is unyielding. As it is contrary to all theory that a vessel filled with fluid and hermetically sealed, should endure the ever varying alternations of temperature without change—neither affected by the summer heat nor winter's cold—it is desirable to give my experience in relation to this subject. I have in my possession preparations sealed originally without a bubble of air; one of these, the vessel being 9 inches by 11, half an inch deep, was made in Edinburgh in 1841, and framed as a picture—to this time it has not a bubble of air.

Some dissections of Terebratula australis—mounted seven years ago, are as perfect now as they were when first completed. These and other preparations, have travelled extensively in England, have crossed the Atlantic where they were well tossed about for two weeks out of seven, and they still accompany me in my wanderings in this country. They endured with impunity the capricious temperature of England, and here they have been subject to 90° down to 8°—still without change, while other preparations, side by side with the former, about which as much care had been bestowed have miserably failed—why is this?

In some instances one can readily perceive that the cause of failure was due to an ill made vessel, or to the want of flatness in the top glass, which, although yielding to the cement in the first instance, has ultimately resumed its figure and gaped at one corner, whereby the fluid can escape. But, as this explanation does not meet every case of failure, I have been induced to make experiments with the preserving fluids respectively, subjecting them to the action of high and low temperatures, and watching the results, and these direct experiments agree exactly with the soundness, or the contrary, of all my preparations. I poured some A1 fluid into a glass tube with a bulb, and adapted it to a thermometer scale. I placed this tube and a thermometer into a vessel of water previously regulated to 60°. With ice and salt I gradually lowered the temperature to zero and noted the amount

of *contraction* of the A fluid. I then as gradually increased the temperature to 100° and noted the *expansion* of the fluid. I went through a similar course of proceeding with the A2 and the B fluids respectively, always taking 60° as the mean, and 100° and zero as the maximum and minimum temperatures.

From these experiments I deduced the following: in all the fluids the contraction was not equal to the expansion; neither were the results similar with any two fluids; that, the expansion was in direct ratio to the density of the fluid; thus, it is least of all in the A2 and greatest in the B1,148 fluid. The A2 fluid is affected so little by temperature, that the experiments seemed to indicate the probability that this fluid might remain permanent in well secured vessels, and I find that all the sound preparations contain this fluid. The B1,100 fluid is not to be trusted in a higher temperature than 80°; and if subjected to such, B1,148 will expand so decidedly and considerably, as to break the top glass (which is always the thinnest) of the preparation. The experiment with the A2 fluid induced me to think that the expansion and contraction of the glass agreed very *nearly*, or *quite*, it might be, with the fluid; and that the marine glue and black cement fully sympathized. I took, therefore, a sound preparation contained in a large cell, the top cemented with the black cement, and placed it in the water at 60°, and from this point I continued the experiment down to zero, up to 100° as before described, and continued to do this alternately for nearly two hours—to this time it has not undergone any change. I have sometimes been asked, "what provision I made against freezing of the fluid;" no provision is necessary, as it cannot freeze. A fluid of the specific gravity, therefore, of the A2 fluid, may be regarded as the maximum density that can weather the temperature of the atmosphere through all its phases, and the knowledge of this fact makes me fear for the permanency of valuable preparations by my hand, preserved in the denser fluids and now in Europe.

Another fact has resulted, that if a preparation be well and thoroughly preserved in either of the fluids before described, and which may be best adapted to the particular animal, it may be sealed down hermetically without air in a fluid of less specific gravity, and it will be permanent.

The square glass boxes are eminently calculated to give the best effects, as preparations, to a vast number of the invertebrate animals. If a bottle be used, no matter what its shape, the operator has no alternative, but must suspend his specimen from the top, whether it be an animal whose natural habitat is the bottom of the ocean fixed firmly to a solid rock or otherwise, there it

must hang mid-way in the fluid like the coffin of Mahomet, 'twixt earth and sky. Surely preparations should be so managed that as far as possible they should tell their own tale, that those persons whose curiosity induces them to scan the contents of a public museum may learn while they look. That a Cephalopod should be seen as if walking on his head; a Tunicated mollusc as if growing from the bottom of the vessel, whilst the beautifully branched Sertularian zoophyte and other animals whose nature it is to cling for support to sub-marine substances should retain, as preparations, all their characteristics in this respect.

In vessels of the form I have described, all such effects can be readily attained, and I have mounted a number of preparations years ago, with special reference to this subject. The naturalist who stuffs birds or beasts, imparts as much of character to his specimens as possible; and the great end that I have always proposed to myself in connection with my glass vessels, is to do as much for those animals that can only be preserved as *wet* preparations, as has hitherto been commonly done with dried specimens, and I believe I have pretty well succeeded.

I have displayed in upright vessels two specimens of Argonauta argo as if walking upon a large piece of branched white coral, the latter being fixed to the bottom of the vessel. Zoophytes attached to shells, or stones, I have displayed as I found them, and so of other animals. * * * *

One of the chief difficulties in the way of examining *wet* preparations, whether in bottles or otherwise, is, that they are literally drowned in light. Now, as every ray of light that is not wanted is in the way, I carefully stop it out. Few preparations have more than *one* really valuable surface—that which in a public museum is placed towards the spectator; by making the opposite surface of the preparation jar black, the light, instead of going through the vessel, is arrested, and all that is superfluous absorbed; the preparation is thrown forward, and there is really in effect more light with which to examine it. The band of black should not be too broad, but nicely apportioned to the requirements of the preparation. Among the few preparations that I mounted in bottles and preparation jars for the museum of the Royal College of Surgeons, I ventured to carry out this idea, and the results fully justify all that I have said or can say. Foremost and most conspicuous amongst those with the black back, is a preparation of the cerebro-spinal axis of man, and the spinal cord of the giraffe—the latter preparation dissected by Prof. Owen, and merely mounted by me—both of which are conspicuously visible at the opposite end of the long gallery in which they are placed. I encountered deeply rooted prejudices, due to an aversion to whatever is new; the introduction of a back ground to

a preparation bottle was a novelty, therefore an innovation, and consequently, was not adopted. And yet the preparations referred to stand in the midst of an extensive series of kindred structures, all of which would be amazingly improved by this simple addition.

In my estimation a back ground is of the greatest importance, and it forms an important part of my plan. With the black cement, sufficiently thinned with turpentine to form a paint, I coat that portion of the under surface of every preparation—great and small—that is not immediately beneath the object. Two or three coats of this paint form a beautiful black, which shows through the upper surface of the glass like highly polished marble, for which substance it has frequently been mistaken. Beneath the preparation I place whatever color best contrasts with it, and for this purpose use paper for all colors but black; sometimes white, pale amber, ultra-marine. The under surface of the glass I defend from scratching by pasting paper over its entire surface, and I reserve access to the obverse of the preparation either for the sake of transmitting light for microscopical purposes, or for examining that particular surface, by leaving the paper which lies beneath the object free on three sides, and by pasting it down on the *fourth*, it can be turned back and replaced at pleasure.

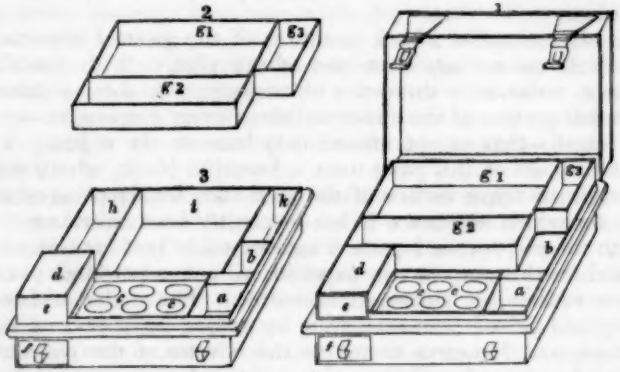
Preparation making is an art demanding much devotion and time to acquire. At present, museums are managed and controlled by men who believe that there is no better plan of proceeding than to put specimens in a bottle with spirit although the liquid soon becomes discolored, and deposits a thick sediment that renders the whole very turbid the instant the bottle is moved; and this is called "a fine preparation," in which opinion those to whom it is submitted generally acquiesce; and this must continue so long as it be the fashion to expend a great sum of money on the *outside* of a museum, and as little as possible within.*

Manipulating Box.—So many little things are required in making preparations, that I determined some years ago to gather them together and contrive a box capable of holding a fair supply of tools and utensils for ordinary work. I drew plans of the several portions of what I considered to be necessary, similar to the details of fig. 13, and I had it made in *tin* as the lightest and strongest material. The actual size of my box, outside measure, is 10 inches by 7, 3½ inches high. No. 1 represents the box when open, No. 2, a movable tray of peculiar form, with three

* Preparations in bottles with stoppers have their use, beyond that of mere exhibition in a museum; for scientific investigation often requires that the specimen be taken from its bottle for particular examination and study.—Eds.

compartments, and No. 3 represents the box when No. 2 is removed from it: the letters from *a* to *f*, indicate the same parts in both figures.

13.



No. 1.—*a*, Compartment to receive a bottle 2 inches square, 3½ high, to the top of the stopper, for preserving fluid.—*b*, Space reserved for the spirit lamp.—*c c*, A shelf of tin perforated with six holes to receive three stoppered, two dram bottles, for liq. potassæ; sulphuric acid; camphene (or turpentine); and three glass jars, 2⅜ths high, ⅞ths diameter, made out of stout glass tube without a lip, and fitted with corks for Canada balsam; prepared asphaltum; lampblack and gold size.—*d*, A slab of porcelain, 2⅜ths square, resting upon a tin frame and carried up so as to be flush with the level of all the bottles, and the tray (2) when in its place. Beneath the slab is *e*, a drawer 2⅜ths long, 2¼ wide and ¼ths deep, to hold about three dozen of the smallest slides I use, viz., 2¼th by ⅜ths. Beneath *e* is a deep well which occupies the space from the drawer *e* to *f*, another drawer, which runs the whole length of the box, from front to back; it has the width and depth of *e*.

No. 2.—g^1, This compartment of the tray measures 8 inches long, 2½ *full*, wide, 1¼ deep. It contains the iron plate, its brass legs and mahogany stand; a small cutting board kept for thin glass only, measuring 6 inches by 2⅜ths, ¼th thick, furnished with a guide board 5 inches long, ¼ inch wide, and ¼th thick, and a guage 6 inches long, nearly ⅜ths wide, and ¼th thick. A card-board box, 2¼ by 1⅜ths and ⅜ths deep, to hold plates of thin glass; the small brass square already described; mahogany square 6 inches by 2½—⅛th thick; a number of badger's hair pencils in handles.—g^2, Glazier's diamond; scratch do.; marine glue (cane) brush; knife and engraver's tool for cleaning cells; small glass muller to grind the black cement on the porcelain slab, and sundry glass (dropping and other) tubes.—g^3, Pill box with whiting; white wax for thread; cotton wool; sundries.

No. 3.—*h*, A fixed tray 4 inches by 2¼ and ⅜ths deep, to contain glass for covers to larger cells.—*i*, A well 5½ by 4 inches, 1¼ *full* deep, to hold spare slides of the larger size, with, or without cells cemented to them—spare cells, &c.—*k*, A supply of the finest and other varieties of China three twist; pill box containing small pins so necessary in dissecting; pill box containing cells cut in the *thinnest* glass.—Drawer, *f*, contains several small palette knives in ivory handles for mixing the cement on the slab; the blades differ in length from 1¼ to 3½th, and from ¼th to ⅜ths at the point; drills for glass and many little things. Below the shelf, *c c*, is a similarly perforated shelf, raised somewhat from the bottom, the design being to grasp the bottles at two points. Should the bottles not be sufficiently high to occupy all the depth allowed for them, they must be raised by a shelf of tin—the intention being that when the box is closed, every thing should be more or less pressed upon and kept in its place. The whole is japanned dead black within and lustrous black without.

ART. XXIII.—*Abstract of Meteorologial Observations, made at Marietta, Ohio*, in Lat. 39° 25′, West Long. 4° 30′ from Washington City, for the year 1851; by S. P. HILDRETH, M.D.

MONTHS.	THERMOMETER.				Fair days.	Cloudy days.	Inches of rain and melted snow.	Winds.	BAROMETER.			
	Mean of 3 observations.	Mean of 2 observ'ns.—highest & lowest.	Maximum.	Minimum.			In. 100		Maximum.	Minimum.	Range.	Mean.
January, -	34·33	35·20	64	-4	25	6	0·50	s., s. w. & w.	30·03	28·80	1·23	29·41
February,	40·70	41·50	66	2	11	17	4·88	s., s. w. & n.	29·95	28·95	1·00	29·45
March, -	46·33	47·20	78	20	21	10	1·44	s., s. e. & n.	29·55	29·00	·55	29·28
April, -	52·33	51·83	80	28	18	12	3·04	n., w., & s. e.	29·75	28·70	1·05	29·23
May, -	61·57	62·47	86	28	19	12	6·00	s. w., n. & e.	29·75	29·12	·63	29·44
June, -	69·00	70·60	88	46	22	8	2·60	s. s. w. & n.	29·70	29·00	·70	29·33
July, -	72·33	72·83	94	52	22	9	2·40	n., s. & w.	29·55	29·20	·35	29·35
August, -	70·27	72·25	87	48	23	8	3·60	n. & s.	29·68	29·25	·43	29·47
September,	66·2-	68·50	96	40	25	5	2·38	s., s. e. & n.	29·80	29·15	·65	29·47
October, -	52·33	53·15	81	28	20	11	1·68	s., s. e. & w.	29·60	29·00	·60	29·30
November,	39·52	40·00	71	18	12	18	4·00	n. & w.	29·80	29·90	·90	29·35
December,	28·00	28·00	65	-3	11	20	2·42	w., s. e. & n.	29·80	29·08	·72	29·44
Mean for year,	52·75	53·62					34·94					

THE mean temperature for the year which has just closed is 52°·75, which is about as usual. The amount of rain has been considerably less than the annual mean for this climate, which is about 42 inches, (but rising occasionally to 53 or 54 inches,) whereas this year it was only 34·94 inches. The years 1834, 1839, and 1845, as well as 1851, were remarkable for the diminished amount of rain. The month of December, as might be expected from the short days and lengthened nights, is our coldest month, and this year has been remarkably severe. A recapitulation of the history of this month for the past twenty-five years, as showing its bearing on the navigation of the Ohio river, may be valuable and interesting in this day of railroad improvements; especially as there is now a road in preparation from Cincinnati to Pittsburgh, along the northern border of the river, pursuing its general direction, a few miles from its waters, which will obviate the serious difficulties now felt from the obstructions to navigation by low water in the summer and autumn, and ice in the winter, viz.:

December, 1825.—Nearly as cold as this year; river closed the 20th of the month; snow a foot deep on the 19th; 6° below zero on the 22d; mean temperature 29°·33.

" 1826.—Ohio closed the 27th to 30th January following; mean temperature of that month 31°·42.

" 1827.—Ohio river open all winter; mean temperature 43°.

December, 1828.—Ohio open all winter; some ice in February; mean temperature 39°·17; for year 55°·22. Rain 49·50 inches.

" 1829.—Ohio closed 27th January, 1830; mean for December 44°·09; mean for the year 55°·22. Rain 39·52 inches.

" 1830.—Mean 35°·74; Ohio full of floating ice the 22d of the month; closed 23d January following, and opened the 20th February; snow fifteen inches in January. Rain for year 37·26.

" 1831.—Mean for month 21°·00; lowest 10°. Rain for the year 53·54 inches. The 8th of this month the Ohio and Muskingum rivers closed, and on the 16th loaded teams crossed with wood, &c.; the ice was from twelve to eighteen inches thick; on the 8th of January following the ice broke up causing great damage to boats, destroying numbers of steamboats, lying at the shore in exposed situations; in February was the great flood of 1832.

" 1832.—Mean 36°; Ohio open all winter. Rain 48·33 inches.

" 1833.—Mean 37°·00; river open all winter. Mean 54°·56.

" 1834.—Mean 36°·66; river closed 6th January, 1835; open again the last of the month; fruit all destroyed this year as in 1851, by frosts in May. Rain for year 34·66 inches.

" 1835.—Mean 31°·00; Ohio closed 28th January following; open the 15th February; thermometer 18° on the 5th. Rain 42·46 inches; mean for that year 50°·65.

" 1836.—Mean 30°·70; navigation obstructed from the 8th December, until the 8th of February. Rain 36·09 inches.

" 1837.—Mean 35°·57; navigation obstructed from 30th January to the 13th of March; February very cold; mean for year 51°·5.

" 1838.—Mean 28°·15; temperature 9 below zero the 31st of the month; the river closed 6th December; opened the 13th January.

" 1839.—Mean 36°·33; Ohio closed the 29th; opened the 15th January; amount of rain 33·32 inches for year.

" 1840.—Mean 30·14; Ohio closed 3d January following and opened the 8th.

" 1841.—Mean 36°·33; no ice this month; open all winter.

" 1842.—Mean 33°·33; much floating ice the 24th November; river closed the 28th; open the 5th December, and the rest of winter.

" 1843.—Mean 35°·00; mean for year 50°·70; open all winter.

" 1844.—Mean 34°·37; rain for year 36·64 in.; open all winter.

" 1845.—Mean 25°·40; at and below zero 2° on four days; Ohio closed the 5th of the month; was open a few days and closed again the 26th. Rain 33·90 inches.

" 1846.—Mean 37°·77; Ohio open all winter.

December, 1847.—Mean 35°·33; rain, December 10th, 10 inches; flood the 15th of the month, covering the bottoms; rain 52·31 inches; the 27th, Ohio full of floating ice, but not closed all winter.

" 1848.—Mean 42°·16; no ice until January; open all winter.

" 1849.—Mean 31°·33; no ice in the Ohio till the last of the month.

" 1850.—Mean 34°·66; rain this month 7 inches; for the year 52·30 inches; rivers open; little ice.

" 1851.—Mean 28°·00; very cold; below zero a number of mornings; Ohio closed the 19th of the month and opened again the 30th.

The mean temperature for the winter months is 36°·56, not far from the mean of our winters.

The mean of the spring months in 1851, is 53°·41. The blossoming of fruit trees was as early as usual, but severe frosts the early part of May destroyed the apple and peach germs through all the southern and eastern portions of Ohio, extending into Virginia and Kentucky. That portion of the state within the influence of the atmosphere of Lake Erie, escaped this calamity; and for the first time, Cincinnati and the towns on the Ohio river, received their apples from New York and the lake shore of Ohio.

The mean of the summer months is 70°·53; the season was temperate, rising only on one day above 90°, on the 26th July reaching 94°. The crops of Indian corn suffered a good deal from drought, from the 22nd August to the 27th September, at a period when this grain needs a full supply of moisture to fill out the kernels; but an abundant rain the last of that month recruited the flagging vegetation. Streams of water and springs were very low in the center of the state, causing a good deal of suffering to cattle and horses, it being difficult in many places to provide a supply for wagoners and stage coaches.

The mean temperature of autumn is 52°·71, which is a little above common years. The hottest weather was felt in September; it rising above 90° on six days—the 12th being at 96°—the greatest heat for many years. The streams continued low all the autumn and the Ohio was not in a fit state for boat navigavion, of produce or flat boats until the 17th November; whereas in most years it rises sufficiently for this purpose in October. A large portion of the produce grown in the vicinity of this river, is taken to markets on the Mississippi by the farmers along its banks, on their own account. As a sample of the vast amount of property afloat on the Ohio, 2000 boats were counted in 24 hours by an ascending steamer, between the mouth of the Scioto and Marietta, a distance of about 200 miles. A large number of

these were loaded with coal from Pittsburgh and Wheeling. A flat boat of ordinary size carries 1500 or 2000 barrels of produce.

The year has been fruitful in cereal productions. Excellent crops of wheat, corn, oats and hay, while the potato has escaped destruction by that odious epidemic called "the rot," and thousands of barrels have gone to the markets on the "father of rivers." Fruits of nearly all kinds failed. Grapes fared better than any other, blooming after the frosts had in a measure ceased. Strawberries were abundant, and melons, until the heat and drought of September killed the vines.

Floral Calendar, &c.—Feb. 23d, Garden crocus in bloom.

March 2nd, Robin appears; 16th, Blackbird; 19th, Fruit buds of the pear and peach swollen and ready to open; 23d, Hepatica triloba in blossom; 28th, Peach and Plum in bloom in warm exposures; 31st, Hyacinth, Dodecatheon media, Pyrus japonica.

April 1st, Pear in blossom; 2nd, Peach and cherry; 4th, Cornus Florida, Judas tree and Sanguinaria canadensis; 5th, cut Rhubarb stems two feet long, protected only by an empty flour barrel; 7th, Wood anemone; 10th, Apple in full bloom; 15th, Hard frost, thermometer 28°, peach killed, some pears escaped; 22nd, set out in the garden, Dahlia tubers, Amaryllis, &c.; 24th, Quince in blossom; 25th, Tulips in full bloom, not much injured by the frost; 28th, Harebell and Uvularia.

May 2nd, Hard frost, thermometer 28°; 3d, Chickasaw plum; 5th, Crab apple; 6th, Hard frost, 29°, ice one-sixth inch on water in a bowl,—corn planted the 9th of April not yet up; 9th, Yellow Cypripedia; 12th, Rose-colored Peony; 15th, Purple mulbery, crimson Peony; 19th, Ribes villosa, and Prunus virginiana; 20th, Syringa aromatica; 21st, Purple tree Peony; 26th, Syringa Philadel., locust blooms very little, hurt by frost; 28th, Garden pea on table.

June 16th, Catalpa tree; 21st, Cucumber raised in open ground protected from frost by boxes, fit for eating.

July 14th, Ribes villosa or blackberry ripe.

Aug. 25th, Brown Beurre pear ripe; 30th, Doyenne pear—these escaped the frost, being protected by buildings near them.

Oct. 24th, First frost to kill Dahlia tops.

Marietta, January 1, 1852.

ART. XXIV.—*Note on Chinese Horology: with suggestions on the form of Clocks, adapted for the Chinese market.* Written for the U. S. Patent Office; by D. J. MACGOWAN, M.D.

A REQUEST made about two years since by the United States Patent Office for information from American citizens resident in China, calculated to be useful to home industry, has not received that attention which it merits, notwithstanding there exists as incentives, on the one hand, the unrenouncable claims of country, and on the other, ample opportunities for complying with that request. Her wide spread territory, the varied productions of her soil, and the high position of China as an agricultural state, lead us to expect that no inconsiderable additions to our own agriculture would result from a careful survey of the various points accessible to foreigners, and it would doubtless be found that many plants indigenous to this soil are capable of being naturalized in one part or another of our continent.

In a manufacturing point of view, although there is much less to repay research, yet there are some branches of industry in this department, the investigation of which could not fail to bring valuable facts to light, and if no more can be done than to point out defects in Chinese labor which our artisans can supply, that alone would prove mutually advantageous to the two great nations on the opposite shores of the Pacific. *Clock-making*, which forms the subject of this note, is a case in point; and it is believed that with a modification to be suggested, American clocks can be made an article of extensive import into China. For a long period the importation of clocks and watches, chiefly the former, into this country from the continent of Europe, was little short of half a million of dollars annually. This trade has nearly ceased, partly owing, no doubt, to the rapid impoverishment of the country by the opium traffic, and partly to the fact that native manufacturers are able to compete with foreigners. Yet clocks are not often met with in China; they are chiefly confined to the public offices, where it is common to find half a dozen, all in a row. The number annually manufactured cannot be large, for in the richest cities of China, clock-makers are not numerous. At Nankin there are forty shops, at Suchau thirty, Hangchau seventeen, and at Ningpo seven. The average number of men employed in each being less than four—who are mostly occupied in *repairing* watches and clocks. The cheapest clock they make costs $7. Some are worth as much as $100, the most common price being about $25 each. A manufacturer estimates the number of clocks made at the above places at one thousand per annum, and probably five hundred more would more than cover the whole annual manufacture of the empire. A few

watches are made, with the exception of chains and springs, which are imported. The oil used by Chinese workmen to abate friction appears to be particularly adapted for that purpose, though expensive; it is obtained from the flowers of the *Olea fragrans.*

Before describing the kind of clock which seems adapted for this market, a brief glance at the history of the horological art in China may not be inappropriate. It had its rise as in the western side of Asia, in the *Clepsydra.*

Assuming, what is in the highest degree probable, the authenticity and accuracy of the Shúking, we find that forty-five centuries* ago, the Chinese had occupied themselves with the construction of astronomical instruments somewhat similar to the quadrant and armillary sphere, and the observations they made with them, even at that remote period, are remarkable for their accuracy, enabling them to form a useful calendar;—the present cycle of sixty was adopted at that time by Hwangti, 2697–2597 B. C. To this emperor is attributed the invention of the clepsydra. The instrument at that period was probably very rude, and not used as a time-piece, but for astronomical purposes, in the same manner as employed by Tycho Brahe for measuring the motion of stars, and subsequently by Dudely in making maritime observations. It was committed to the care of an officer of rank, styled Clepsydra Adjustor.

The greatest philosopher in Chinese history anterior to Confucius was Duke Chau, the alleged inventor of the compass; he appears also to have been the first to employ the clepsydra as a time-piece. He divided the floating index into one hundred equal parts or "*Kih.*" In winter, forty kih were allotted to the day and sixty to the night, and in summer this was reversed. Spring and autumn were equally divided. This instrument was provided with forty-eight indices—two for each of the twenty-four terms of the year. They were consequently changed semi-monthly, one index being employed for the day, and another for the night. Two were employed every day probably to remedy in a measure the obvious defect of all clepsydras—of varying in the speed of their rise or fall, according to the ever varying quantity of water in the vessel; which might be done by having the indices differently divided. To keep the water from freezing in winter, the instrument was connected with a furnace and surrounded by heated water. Chau flourished eleven centuries

* Chronology.—Although doubts may exist respecting the absolute accuracy of Chinese chronology, it must nevertheless be admitted, that it is so far correct as to render arguments founded on the commonly received chronology altogether untenable, and it is matter of regret, therefore, that the latter has been followed in their Chinese publications by all Romish and Protestant missionaries. I cannot too earnestly urge the adoption of Hale's chronology, and that speedily, lest in the mean time some Chinese Celsus or Porphyry should arise and bring objections against our faith not easily answered, to the satisfaction of their countrymen.

before our era. The forms of the apparatus have been various, but they generally consisted of an upper and a lower vessel, always of copper, the former having an aperture in the bottom, through which water percolated into the latter, where floated an index, the gradual rise of which indicated successive periods of time. In some, this was reversed, the float being made to mark time by its fall. A portable one was occasionally employed in ancient times on horseback, in military tactics.

Instruments constructed on the same principles with the above were in use among the Chaldeans and Egyptians at an early period; that of Clesbius of Alexandria being an improvement over those of more ancient times. The invention of Western Asia, was doubtless wholly independent of that of the East, both being the result of similar wants. Clepsydras were subsequently formed of a succession of vessels communicating by tubes passing through birds, dragons, &c., which were rendered still more ornamental, by the indices being held in the hands of genii.

The earliest application of motion, to the clepsydra, appears to have been in the reign of Shuen-ti (126—145 A.D.) by Tsiang-hung, who constructed a sort of orrery, representing the apparent motion of the heavenly bodies around the earth, which was kept in motion by dropping water. There is reference also to an ininstrument of this description in the third century.

In the sixth century an instrument was in use which indicated the course of time by the weight of water as it gradually came from the beak of a bird and was received on a vessel in a balance, every pound representing a *kih*. About this time, mercury began to be employed instead of water, which rendered the aid of heat in winter unnecessary. Changes were made also in the relative number of *kih* for day and night, so as to vary with the seasons.

As in Europe, monks of the Roman church devoted considerable attention to mechanical inventions, especially in the construction of instruments for measuring time, for the regulation of their worship, and vigils; in like manner also, Budhist monks in their silent retreats, but at an earlier period, similarly occupied themselves and for the same purpose. Several instruments designed as time-pieces, the inventions of priests, are mentioned in Chinese history. They present nothing novel, however, with the exception of one, which was nothing more than a perforated copper vessel placed in a tube of water, which gradually filled and sunk every hour, requiring of course frequent attention.

Although their knowledge of hydrodynamics has ever been very limited, the Chinese appear to have been the first to devise that form of clepsydra to which the term water-clock is alone properly applied, that is to say, composed of apparatus which rendered watching unnecessary by striking the hours. Until the commencement of the eighth century, the persons employed to watch

the clepsydra in palaces and public places, struck bells or drums every *kih*, but at this period a clock was constructed, consisting of four vessels, with machinery which caused a drum to be struck by day, and a bell by night, to indicate the hours and watches. No description of the works of this interesting invention can be found. It is possible, however, that the Saracens may have anticipated them in this invention of water-clocks.

In the history of the Tong dynasty (620—907) it is stated that in the Fahlin country, (which in this instance doubtless means Persia, though the best living authority amongst the Chinese makes it Judea,) there is a clepsydra on a terrace near the palace, formed of a balance which contained twelve metal or gold balls, one of which fell every hour on a bell, and thus struck the hours correctly. It is not improbable that this instrument is identical with the celebrated one which the king of Persia sent in 807 to Charlemagne.

In 980 an astronomer named Tsiang, made an improvement on all former instruments, and which, considering the period, was a remarkable specimen of art. The machine, which was in a sort of miniature terrace, was ten feet high, divided into three stories, the works being in the middle. Twelve images of men, one for every hour, appeared in turn before an opening in the terrace. Another set of automata struck the twelve hours and the *kih* or eighths of such hours. These figures occupied the lower story. The upper was devoted to astronomy, where there was an orrery in motion, which it is obvious must have rendered complex machinery necessary. We are only told that it had oblique, perpendicular, and horizontal wheels, and that it was kept in motion by falling water.

As the Saracens had reached China by sea, at the close of the eighth century, and by land at an earlier period, some assistance may have been derived from them in the construction of this instrument, but I am disposed to consider it wholly Chinese. Beckman, after much learned research, ascribes the invention of clocks to the Saracens, and the first appearance of these instruments in Europe to the eleventh century.

Mention may here be made of other instruments of the same description, also constructed about this period. One, which like the last, united an orrery and clepsydra, was formed in one part like a water lily, whilst in another were images of a dragon, a tiger, a bird and a tortoise, which struck the *kih* on a drum, and a dozen gods, which struck the hours on a bell; with various other motions, besides a representation of the revolution of the heavenly bodies. The machinery of another of these was moved by an undershot water-wheel, its axis was even with the ground, and consequently the frame containing it was partly below the surface. The motions of the sun and moon, stars and planets, were made

to revolve around a figure of the earth, represented as a plain, from east to west. Images of men struck the hour, and its parts. In this, however, as in all the aforenamed instruments, the sounds struck were doubtless always the same, as the Chinese do not count the hours. Another machine was constructed which also represented the motions of the heavenly bodies. It was a huge hollow globe, containing lights, and perforated on its surface, so as to afford in the dark a good representation of the heavens. This also was set in motion by falling water. Subsequent to this, various machines are mentioned, but the brief notices given afford nothing of interest until we approach the close of the Yuen dynasty, the middle of the fourteenth century. Shung-tsing, the last of the race of the great Genghis Khian, depicted in history as an effeminate prince and as having the physiognomy of a monkey, was evidently a man of great mechanical skill; and to the last, when his dominions were slipping from him, and confusion reigned everywhere, he amused himself by making models of vessels, automata, and time-pieces. His chief work was a machine contained in a box, seven feet high, and half that in width, on the top of which were three small temples. The middle of these temples had fairies holding horary characters, one of which made her appearance every hour. Time was struck by a couple of gods, and it is said they kept it very accurately. In the side temples were representations of the sun and moon respectively, and from these places genii issued, crossing a bridge to the middle temple, and after ascertaining, as it were, the time of day from the fairies, returning again to their quarters. The motions in this case were, it is thought, effected by springs. An instrument somewhat similar is described as an ornament, in the palace of the capital of Corea; it was a clepsydra with springs, representing the motions of the celestial orbs, and having automata to strike the hour. Since the introduction of European clocks, clepsydras have fallen into disuse. The only one perhaps in the empire, is that in the watch tower in the city of Canton; it is of the simplest form, having no movements of any kind, but it is said to keep accurate time. The Chinese automata, so much admired, are, in their internal structure, imitations of foreign articles, as their name implies.

In *dialling*, the Chinese have never accomplished anything, being deficient in the requisite knowledge of astronomy and mathematics. It is true, the projection of the shadow of the gnomon was carefully observed at the earliest historic period, but this was for astronomical purposes only.* Proper sun dials were

* It was by a gnomon that the ancient Chinese endeavored to ascertain the centre of the earth. A measurement of the length of the solstitial shadow, made at Loyang, on the Yellow River, 1200 B. C., was found by Laplace (quoted by Humboldt, in Cosmos, vol. ii, p. 115) to accord perfectly with the theory of the obliquity of the ecliptic, which was only established at the close of the last century.

unquestionably derived from the West. But they were *not* introduced, as Sir J. F. Davis supposes, by the Jesuits; the Chinese are probably indebted to the Mahomedans for this instrument, although we find an astronomer endeavoring to rectify the clepsydra by means of the sun's shadow projected by a gnomon, about a century earlier than the Hegira. There is a sun dial in the imperial observatory at Pekin above four feet in diameter. Smaller ones are sometimes met with in public offices. These were all made under the direction of missionaries of the Roman church, or their pupils. From remote antiquity, a family named Wang, residing in Hiuning, N. lat. 29° 53′, long. E. Greenwich 118° 17′, in the province of Ganhwui, has had the exclusive manufacture of pocket compasses, with which sundials are often connected. In most of these, a thread attached to the lid of the instrument serves as a gnomon, without any adaptation for different latitudes, although they are in use in every part of the empire. Another form, rather less rude, is employed by clockmakers for adjusting their time pieces; it is marked with notches, one for each month of the year, to give the gnomon a different angle every month. The Japanese instrument exceeds that of China in every respect.

Time is not unfrequently kept by igniting incense sticks, the combustion of which proceeds so slowly and regularly as to answer for temporary use tolerably well.

Hour glasses are scarcely known in China, and only mentioned in dictionaries as instruments employed in western countries to measure time.

A native writer on antiquities says, "the western priest Límátau (M. Ricci,) made a clock which rendered and struck time a whole year without error." The clock brought out by Ricci, if not the first seen in China, is the earliest of which mention is made in Chinese history. They subsequently became an article of import, and as already mentioned, this branch of trade was at one time of considerable value. Clocks and watches of very antique appearance are often met with, specimens of the original models scarcely to be found in any other country; some of the latter by their clumsy figure remind one of their ancient name "Nuremberg eggs," but their workmanship must have been superior to that of most modern ones, or they would not be found in operation at this late day.

The Chinese must have commenced clock making at an early period, as none now engaged in the trade, can tell when or where it originated; nor can it be easily ascertained whether their imitative powers alone enabled them to engage in such an undertaking, or whether they are indebted to the Jesuits for what skill they possess. It is certain that the disciples of Loyala had for a long time, and until quite recently in their corps at Pekin, some who were machinists and watchmakers. One of these *horolo-*

gistes complains in *Lès Lettres Edifiante et Curieuse*, that his time was so occupied with the watches of the grandees, that he had never been able to study the language. Doubtless the fashion which Chinese gentlemen have of carrying a couple of watches which they are anxious should always harmonize, gave the fathers constant employment. A retired statesman of this province has published a very good account of clocks and watches, accompanied with drawings representing their internal structure in a manner sufficiently intelligible.

The Chinese divide the day into twelve parts, which are not numbered, but designated by characters, termed, rather inaptly, horary. These terms were originally employed in forming the nomenclature of the sexegenary cycle, (2657 B. C.) which is still in use. It was not until a much later period, that the duodecimal division of the civil day came into use, when terms to express them were borrowed from the ancient calendar. The same characters are also applied to the months. The first in the list (meaning Son,) is employed at the commencement of every cycle, and to the first of every period of twelve years, and also to the commencement of the civil day—at 11 P. M.—comprising the period between this and 1 A. M. The month which is designated by this term is not the first of the Chinese year, but singularly enough coincides with January. Each of the twelve hours are divided into eight *kih*, corresponding to quarter hours. This diurnal division of time does not appear to have been in use in the time of Confucius, as mention is made in the spring and autumn annals of the ten hours of the day, which accords with the decimal divisions so long employed in clepsydras, the indices of which were uniformly divided into one hundred parts. A commentator of the third century of our era, in explaining the passage relating to the ten hours, adds a couple more, but even at that time, the present horary characters were not employed.

The diagram, Plate I, represents the form I would recommend as suitable for the dial plates of clocks manufactured for this market. The small characters on the outer circle are numerals, exactly corresponding to the Roman figures on western clocks. The inner circle contains the twelve horary characters, and within these are the signs for noon, evening, midnight, and dawn. In the horary circle the large single characters represent whole hours, and the small double ones half hours, equal to a whole European hour.

Let the minute hand extend to the inner part of the outer circle, and make *twelve* revolutions in a diurnal period. The hour hand should reach to the inner edge of the horary characters, and make *one* revolution in the same period of time. Let the pendulum vibrate seconds as now, and the minute hand at the expiration of sixty seconds make half a revolution. It should

strike from 1 to 12 A. M. and P. M., and correspond in this respect to European clocks. It will be understood then, that at our *even* hours, the short hand will point to a large horary character,—
—the middle of a Chinese hour,—and the long hand will be *directly upward;* and at our *odd* hours the former will be opposite the small characters, which point the commencement of their hour and the latter will point *directly downwards*, or at the 12 P. M. of our clocks. Or to repeat the same in another manner,—at 1 o'clock P. M., our reckoning, the hour hand will be half way between the large character at the top and the next one to the right, and the minute hand having made half a revolution, will point perpendicularly downwards and the clock strike one. At the expiration of another of our hours, a whole Chinese hour will have expired when the former hand will have reached the first large character to the right and the latter be directed to the zenith, the clock striking two.

After this, perhaps unnecessarily minute description of what is wanted in the machinery, a few words remain to be added, respecting the instrument as a whole. In the first place, it should be *well* made. A few worthless ones would damage the business irreparably. They should be of brass, and placed in frames of wood, which will not be easily affected by atmospheric changes. Common pine wood veneered with mahogany, have answered well. Spring clocks will not succeed. Some of this description lately sent from New York cannot be kept in repair, whilst a quantity of clocks moved by weights manufactured chiefly in Connecticut, imported into China above seven years ago, have proved good time pieces and give no trouble.

With regard to the external appearance on which so much depends, I would advise, that in every case, there be as much of the works exposed as possible, through an opening in the dial plate. A Chinaman not only wishes to see what he is buying, but what is going on in his instrument when bought, and as his countrymen have the merit of being extreme utilitarians, mirrors in the lower part of the door will be generally preferred to any other ornament. Some, however, should be ornamented at this point for the sake of variety, and perhaps nothing would please more than such a grouping of objects by the artist, as would represent a river, bringing into view a steamboat and a sloop, and on the banks a railroad, locomotive and cars, a steepled church or many storied hotel in the distance, and a stage coach also. Or another interesting device would be afforded by a representation of the solar system, but this would need to be accompanied with several Chinese characters.

It is of primary importance that a particular description of the manner of using the clock, the mode of putting it up, setting it off, winding, regulating, &c., should be given. These directions,

which should be more minute than if designed for English readers, can be translated and printed very easily in this country. But there would be no difficulty in printing the directions by means of wooden blocks in the manufactory at home. In copying the characters for the astronomical characters or descriptions of any kind which may be needed by individuals trying the experiment of clock making for China, I shall furnish most cheerfully for the privilege of increasing the utility of the instrument by introducing with them a few passages of sacred scripture.

It may be asked, why if such a clock be needed by the Chinese, they have never constructed one for themselves? It is certainly marvellous that they should manufacture clocks, including dial plates, and always employ Roman figures, and follow the reckoning of foreigners, which so few of them are able to comprehend, and which by all are considered mysterious and outlandish. It is only to be accounted for on the ground of their limited inventive abilities and high powers of imitation. That a time-piece of this description would be in demand in China, I am perfectly satisfied, from inquiries made of natives in various quarters. Chinese merchants say that they should be retailed at about $5 or $6 each. If I recollect rightly they can be made in Connecticut at $2.50, which would afford sufficient profit both for the mechanic and the merchant.

Ningpo, July 4th, 1851.

ART. XXV.—*On Ventilation by the Parlor Fire;* by WILLIAM HOSKING, Esq., Professor of Architecture and of Engineering Constructions at King's College, London.*

THE term ventilation does not strictly imply what we intend by its use in reference to buildings used as dwelling-houses, or otherwise for the occupation of breathing creatures. To ventilate is defined "to fan with wind;" but one of the main objects for which houses and other enclosed buildings are made, is shelter from the wind. Inasmuch, however, as the wind is but air in motion, and we can only live in air, air may not be shut out of our houses, though, for comfort's sake, we refuse to admit it in the active state of wind. But in doing this—in shutting out the wind—we are apt to put ourselves upon a short allowance of air, and to eke out the short allowance by using the same air over and over again.

There is a broad line of distinction, indeed, to be drawn between in-door and out-door ventilation; for although the principles upon which nature proceeds are the same, the operation is

* Read at the Royal Institution of Great Britain.

influenced by the circumstances under which the process may be carried on. Whether it be on the hill-side, open to the winds of heaven, or in a close room, from which all draught of air is excluded, the expired breath, as it leaves the nostrils heated by the fire in the lungs, rises, or seeks to rise, above their level, and may not be again inhaled. Out of doors the cooler or less heated air of the lower level presents itself for respiration unaffected by the spent exhaled air, but in a close apartment, the whole body of included air must soon be affected by whatever process any portion of it may have undergone. The process by which nature carries off spent air, purifies, and returns it uncontaminated, is thus checked by the circumstances under which we place ourselves within doors. All our devices for shelter from the weather, and for domestic conveniences and comfort, tend to prevent the process provided by nature from taking effect according to the intention in that respect of the Creator. We not only confine ourselves, indeed, and pen up air in low and close rooms, but we introduce fire by which to warm the enclosed air; wanting light within our dwellings when daylight fails, we introduce another sharer in the pent-up air of our rooms, being fire indeed in another form, but generally under such circumstances, that it not only abstracts from the quantity, but injures the quality of what may remain. But fire, whether in the animal system, in the grate, or in the lamp, cannot long endure the imagined limitation of air. There must be access of air—of vital air—by some channel or other, or the fire will go out.

An open fire in the grate must however have a vent for some of its results, or it will be so disagreeable a companion that its presence could not be endured, even as long as the most limited quantity of air would last; and the fire will compel the descent of air by the vent commonly supplied under the name of a flue —a chimney flue—to render its presence tolerable in a closed room, if a supply be not otherwise attainable. But as the outer air at the higher level of the top of a chimney, because of the rarity of the air in and above the flue, responds to the demand of the fire less easily than the lower air, or that at and about the level of the fire; and the lower air, or air at the lower levels, forces its way in, therefore, by any opening it can find or make —through the joints of the flooring-boards, and under the skirtings—the supply passing first up or down the hollow lathed and plastered partitions, sometimes even up from the drains; and through the joints under and about the doors and windows. If these channels do not exist, as they may not when the joiners' work and the plastering are good, or when the open joints referred to are stopped up by any means, the fire smokes, and every known means of curing the chimney failing, means are sought of obtaining heat without the offending fire. Ventilation is not thought of yet.

The open fire may be made to give place to the close stove or to hot-air pipes, to hot-water pipes, or to steam-pipes—which make hot the air about them in a close room without causing draughts. But the warmth obtained in pipes is costly under any circumstances. Air does not take up heat freely, unless it be driven and made to pass freely over the heated surface; and there being little or no consumption of air, and consequently little or no draught, in connection with heated bodies, such as close stoves and hot pipes, the heat from them is not freely diffused, and is not wholesome. There is with all the expense no ventilation.

Stoves and hot pipes are, moreover, exceedingly dangerous inmates in respect of fire. Such things are the most frequent causes, directly or indirectly, of fires in buildings. Placed upon, or laid among or about the timbers and other wood-work of hollow floors, and hollow partitions, and in houses with wooden stairs, more conflagrations are occasioned by hot pipes and stoves, than by anything else, and perhaps more than all other things together.

Open stoves with in-draught of air warmed by being drawn quickly (when it is drawn quickly) over heated surfaces may be made part of a system of safe and wholesome in-door ventilation; but to be perfect there must be also out-draught with *power* to compel the exit of spent or otherwise unwholesome air. But the arrangements for and connected with such stoves are special, and therefore costly, unless the buildings in which they may be employed have been adapted in building to receive them. An in-draught stove may, however, be applied with great advantage as it regards the general warmth and ventilation, in the lowest story of any house, if there be compelled out-draught at the highest level to which it will naturally direct itself if it be not retained, so that the in-draughted air, tempered as it enters, may be drawn out as it becomes spent, or otherwise contaminated.

But this must be considered in all endeavors to effect in-door ventilation, or the endeavor will fail. *The air must be acted upon, and not be left, or be expected, to act of itself, and to pass in or out as may be desired, merely because ways of ingress and egress are made for it.* Make a fire in a room, or apply an air-pump to the room, and the outer air will respond to the power exerted by either by any course that may be open to it, and supply the place of that which may be consumed or ejected; but open a window in an otherwise close room and no air will enter; no air can enter, indeed, unless force be applied as with a bellows, whereby as much may be driven out as is driven in, with the effect only of diluting not of purifying. Even at that short season of the year in which windows may be freely opened, unless windows are so placed as to admit of the process of out-door ventilation being carried on through them by a thorough draught

from low levels to high levels, open windows are not sufficient to effect thorough in-door ventilation. There must for this purpose be in every room a way by which a draught can be obtained, and this draught must take effect upon the most impure air of the room, which is that of the highest level. The chimney opening may supply a way at a low level, and a draught may be established between it and the window, but the air removed from the room by such a draught is not necessarily the spent or foul air. But make an opening into the chimney flue near the highest level in the room, that is to say, as near as may be to the ceiling, and if a draught be established between the window and the flue by this opening, the ventilation is complete; that is to say again, if there be draught enough in the chimney flue from any cause to induce an up-current through it, or if there be motion of the external air to drive the air in at the window and force an up-current through the flue.

Windows may not be put open in the long enduring colder season, however, and for the same reason in-draughts of the outer air by any other channel are offensive and injurious. To open a door for the sake of air is but a modification of opening a window, and, if the door be an internal one, with the effect of admitting already enclosed, and, probably, contaminated air. Means of efficient in-door ventilation must therefore be independent of windows and doors; and the means should be such as will lead to a result at once wholesome and agreeable.

Many plans have been suggested, and some have been carried into effect, of warming air, and then forcing it into or drawing it through buildings, and, in the process of doing so, removing the foul or spent air from the apartments to which it may be applied. Some of these plans are more and some are less available to wholesome and agreeable in-door ventilation, but even the best are rather adapted to large apartments, such as those of hospitals, churches, theatres, and assembly-rooms, than to private dwelling-houses in which the rooms are small and labor and cost are to be economized.

Plans have been proposed, too, for the economical ventilation of dwelling-houses: but they seem to be all in a greater or less degree imperfect. Ways of access are provided in some cases for the outer air directly to the fire in every apartment, to feed the fire, and indirectly to ventilate the room; way of egress in addition to the chimney opening and the chimney flue being sometimes provided for the spent air of a room; sometimes, indeed, as before indicated, by an opening into the chimney flue near the ceiling. A direct in-draught of cold air is not agreeable, and it may be pernicious, but if the outer air become warm in its way to the inmates of the room, the objection to its directness ceases. If however the warmth is imparted to it with foul-

ness, the process does not fulfill the condition as to wholesomeness, and this is the case, when the outer air is admitted at or near to the ceiling to take up warmth from the spent and heated atmosphere of the higher levels. Having undergone this process, it is not the fresh air that comes warmed to its inmates, but a mixture of fresh and foul air that cannot be agreeable to any inmate conscious of the nature of the compound.

The endeavor on the present occasion was to show how the familiar fire of an apartment may be made to fulfill all the conditions necessary to obtain in-door ventilation, to the extent at least of the apartment in which the fire may be maintained, and while it is maintained.

A fire in an ordinary grate establishes a draught in the flue over it with power according to its own intensity, and it acts with the same effect, at least, upon the air within its reach, for the means which enable it to establish and keep up the draught in the flue. The fire necessarily heats the grate in which it is kept up, and the materials of which grates are composed being necessarily incombustible, and being also ready recipients and conductors of heat, they will impart heat to whatever they may be brought into contact with them.

It is supposed that the case containing the body of the grate is set on an iron or stone hearth in the chimney recess, free of the sides and back except as to the joints in front. Let all communication between the chamber so formed about the back and sides of the grate and the chimney flue be shut off by an iron plate, open only for the register flap or valve over the fire itself. External air is to be admitted to the closed chambers thus obtained about the grate by a tube or channel leading through the nearest and most convenient outer wall of the building and between the joists of the floor of the room, to and under the outer hearth or slab before the fire, and so to and under the back hearth in which sufficient holes may be made to allow the air entering by the tube or channel to rise into the chamber about the fire-box or grate. Openings taking any form that may be agreeable are to be made through the cheeks of the grate into the air-chamber at the level of the hearth. In this manner will be provided a free inlet for the outer air to the fire-place and to the fire, and of the facility so provided the fire will readily avail itself to the abolition of all illicit draughts. But the air in passing through the air-chamber in its way to the fire which draws it, is drawn over the heated surfaces of the grate and it thus becomes warmed, and in that condition it reaches the apartment.

An upright metal plate set up behind the openings through cheeks of the grate, but clear of them, will bend the current of warmed air in its passage through the inlet holes, and thus compel the fire to allow what is not necessary to it to pass into the

room; and if the opening over the fire to the flue be reduced to the real want of the fire, the consumption of air by the fire will not be so great as may be supposed, and there will remain a supply of tempered air waiting only an inducement to enter for the use of the inmates of the apartment. An opening directly from the room into the flue upon which the fire is acting with a draught more or less strong, at a high level in the room, will afford this inducement; it will allow the draught in the flue to act upon the heated and spent air under the ceiling, and draw it off; and in doing so will induce a flow of the fresh and tempered air from about the body of the grate into the room.

The mode thus indicated of increasing the effect of the familiar fire, and making it subservient to the important function of free and wholesome ventilation, is not to be taken as a mere suggestion, and now for the first time made. It has been in effective operation for six of seven years, and is found to answer well with the simple appliances referred to. But it is the mode and the principle of action that it is desired to recommend, and not the appliancess, since persons more skilled in mechanical contrivances than the author professes to be, may probably be able to devise others better adapted to the purpose.*

The mode referred to of warming and ventilating apartments by their own fires is most easy of application, and in houses of all kinds, great and small, old and new, and as the warmth derived from the fire in any case, comes directly by the in-draughted air, as well as by radiation of heat into the air of the apartment, fuel is economized. If the register flap be made to open and shut, by any means which give easy command over it, so that it may be opened more or less according to the occasion, and this be attended to, the economy will be assured; for it is quite unnecessary to leave the same space open over the fire after the steam and smoke arising from fresh fuel have been thrown off, as may be necessary immediately after coaling. The opening by the register valve into the flue may be reduced when the smoke has been thrown off, so as to check the draught of air through the fire, and greatly to increase the draught by the upper opening into the flue, to the advantage of the ventilation and to the saving of fuel, while the heat from the incandescent fuel will be thereby rather increased than diminished.

Moreover the system being applicable in the cottage of the laborer, as fully and easily as in the better appointed dwellings of those who need not economize so closely as laboring people are obliged to economize, the warmed air about the grate in a lower room may be conveyed directly from the air-chamber about the

* The appliances used by Mr. Hosking will be found more fully described in his "Healthy Homes," published by Mr. Murray.

grate by a metal or pot pipe, up the chimney flue, and be delivered in any upper room next to the same flue and requiring warmth and ventilation, the process of ventilation applied to the lower room being applicable to the upper room also.

The indicated means by which winter ventilation is obtained are not of course equally efficient in summer, for the draught of the fire is wanting; but the inlet at the lower level for fresh air, and the outlet for the spent air at the upper level continuing always open, the heat which the flue will in most cases retain through the summer aided by that of the sun's rays upon the chimney top, secures a certain amount of up-draught, which is not without its effect upon the in-draught by the lower inlet even when windows and doors are shut.

While it is obvious that the air drawn into any house for the purpose of in-door ventilation need not be other than that which would enter by the windows of the same house, it may be necessary to enter into an inquiry as to the condition of the air heretofore spoken of as fresh and pure. "Fresh" and "pure" applied to air must be taken to mean the freshest and purest immediately obtainable, and that will be the same whether it be drawn in through a grated hole in a wall, or by a glazed opening closed by it in the same wall. But it is a fair subject for inquiry, whether,—speaking in London to Londoners,—the air about our houses in London is as pure,—or as free from impurity —as it might be.

The out-door ventilation of large towns may be taken to be more complete above the tops of the houses and of their chimneys than it is, or, perhaps, can be among and about the houses. The processes of nature are there not only unchecked, but are in fact aided by the heat thrown up by the chimneys into the upper air, and impurities which can be passed off by chimney flues, will be more certainly and more effectually removed and changed by nature's chemistry than if they are kept down to fester under foot and to exhale in our streets and about our doors and windows.

At this time every endeavor is made to provide for removing from our dwellings all excrementitious matter, and all soluble refuse, by drains into sewers, and so by the sewers to some outfall for discharge. The drain necessarily falls towards the sewer, and the sewer again to its outfall, and the sullage or soil drainage being rendered liquid thus passes in the usual course. But the usages and the necessities of civilized life cause a large proportion of the liquid refuse from dwelling-houses to pass off in a heated state, or to be followed by hot water arising from culinary processes, and from washing in all its varieties. The heat so entering the drains causes the evolution of fetid and noxious gases

from the matters which go with, or have gone before, the hot water; and with these gases house-drains almost always, and sewers commonly, stand charged. They are light fluids and do not go down with the heavy liquid matters from which they have been evolved, but they seek to rise, and constantly do rise in almost every house through imperfections or derangements of the flaps and traps which are intended to keep them down, but which only, when they do act, compel some of the foul air to enter the sewers, and there to seek outlet to the upper air which they find by the gulley gratings in the streets.

It can hardly be said perhaps that *too much* attention has been given of late to the scour of sewers by water; but it is most certain that too little attention has been given to the considerations last stated, for nothing has been done to relieve the drains and sewers of their worst offence. The evolution of foul and noxious gases in the *drains* is certainly not prevented by scouring the *sewers*. In the meantime the poison exists under foot, and exudes at every pregnable point within and about our houses, and it rises at every grating in our streets, though the senses may become dull to them by constant suffering.

Now this is an evil which can be greatly ameliorated, if it cannot indeed be wholly cured; but it is by a process that, to be effective, must be general, and, therefore, it must be added, compulsory. The process is of familiar application in the ventilation of mines, and particularly of coal mines. An up-cast shaft containing a common chimney flue carried up at the back of every house, and connected with the house-drains at their highest level, would give vent to the foul air in the drains, and discharge it into the upper air. The foul air evolved by heat expands, and expanding it rises, and rising it would be followed by cold air settling down by the gulley gratings in the streets, thus constituting their inlets down-cast shafts, and the sewers and drains themselves channels for the currents setting to the up-cast shafts, by which they would be relieved. The down draft into the sewers would carry with it much soot and fine dust, which would settle upon the liquid current and pass off with it, and so remove some of the tangible as well as the intangible impurities, before referred to, from the air in our streets and about our houses.

Much in this way might be effected by the aid of causes in constant operation; but if the up-cast shaft to every house were also a fire-flue, or were only aided by the draught of a neighboring fire, the up-current would be sufficient not only to prevent the house-drains from retaining foul air, but the foul air would be thrown off into the upper air with better effect and be dissipated innocuously and without offence instead of steaming as it now does from the sewers into the air where it cannot be avoided.

ART. XXVI.—*The Economical Constant Battery;* by Prof. CHAS. G. PAGE, M.D., Washington, D. C.

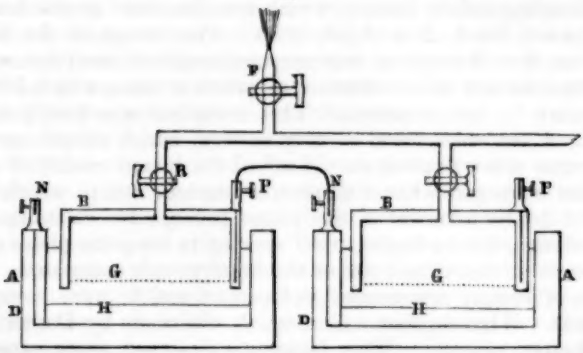
IN the latter part of the year 1837, I invented a modification of Kemp's galvanic battery, which was described in this Journal, vol. xxxvi, No. 1, Jan.–April, 1839. The design of the funnel tube in that description was purposely omitted, as it formed the principal feature of an invention of practical value which I hoped to secure by letters patent. This invention was briefly as follows: A convenient and cleanly battery, which should save the hydrogen when desired, should afford the means readily of igniting the hydrogen when a flame was wanted, and in which there should be no removal of the plates (except for cleaning or replenishing), the hydrogen itself serving to keep the plates out of action even when the poles of the battery were connected. The forms of battery represented in figures 1 and 2, were completed in 1838. That represented in fig. 2, was made by Daniel Davis of Boston, to order. This latter was designed more especially as a substitute for the hydrogen lamp, where the jet is ignited by spongy platinum. The little cage of spongy platinum attached to those lamps is so liable to injury in various ways, and frequently so difficult to supply, that the ignition by the electric spark appeared to be an important improvement to the instrument. After constructing and testing one of these instruments, Mr. Davis informed me by letter, that it involved one difficulty, viz., too much time was required to bring the plates into action, for the purpose of obtaining the spark. I was well satisfied, however, that this was a smaller evil than the frequent loss and expense of the platinum sponge. The form of battery shown in fig. 1, was that adopted for larger operations and the general purposes of galvanic batteries. It is, however, equally well suited, under certain circumstances, to the purposes of the other instrument. The difference between them in this respect is, that in figure 1 battery, the zinc is amalgamated, and in the other it is not. In the first, hydrogen will be supplied only when the poles are joined, and in the second, the hydrogen will be given off at all times except when the acidulated water is below the mouth of the inner receiver.

Fig. 1, exhibits two of the economical batteries constructed upon the basis of Kemp's battery, and involving also the principle of Smee's battery. A is a square box of wood, made tight by pouring into it a quantity of warm shellac* varnish, and pouring it off after the wood has absorbed a sufficient quantity. It

* I have found that a well made box of soft wood may be made very tight by this process, and will last a long time. After the shellac has become thoroughly dry inside, the outside may be painted or varnished.

is an interesting fact, that if the box is varnished or painted outside before the varnish is applied inside, it cannot easily be made tight. Although the rationale is plain, yet the almost universal practice is to cover the outside first. B is an inverted

1.



box of wood prepared in the same manner. G is the negative or conducting plate of the battery, made of wire gauze or a perforated plate. In my first batteries these plates were made of copper, but after the introduction of Smee's battery, I felt satisfied that with Smee's improvement, this form of battery would take precedence of all others where a considerable power was wanted. When copper plates were used, I found that at least two pairs of plates of about ten inches square were necessary to furnish hydrogen fast enough, when the resistance to the current was not very great, to supply a steady flame from the jet. But with a Smee-plate, half the size would have been sufficient. The best material I have found for the plate G, is a perforated plate of platinum platinized; but as this is so expensive, it becomes important to devise some cheaper substitute. Wire gauze of any kind is expensive, and it occurred to me that by precipitating copper upon coarse muslin or millinet, that this might be silvered and then platinized so as to answer. Mr. Mathiot, of the Coast Survey, who has great skill in the management of batteries and the art of electrotyping, informs me that it is perfectly practicable. It is worthy of note here, that Mr. Mathiot has adopted this battery in some of his experiments entirely from his own suggestions, and it is gratifying to me to learn that he thinks highly of it for many purposes. The kind used by Mr. Mathiot is that shewn in fig. 1, except that his vessels are of glass instead of wood, and he had made no provision for leading off or using the hydrogen.

H is the amalgam of zinc.

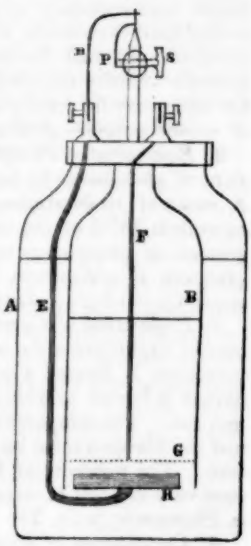
D its connexion with the wire from N.

P, N, the poles of the battery.

The connection with the zinc is made by a wire passing down through the wood, and the connection with the negative plate in a similar manner. The gas pipes R entering the top of the boxes terminate in a common jet F', where the hydrogen may be burned or led off to a gasometer. If the stop-cocks are supposed to be all open and the battery at work, when the atmospheric air has been driven off and hydrogen comes off at F', the jet may be kindled by a spark from the battery produced by breaking the circuit over the jet. It succeeds best if a helix of a magnet is included in the circuit. When the use of the battery is to be discontinued, the stop-cock F' is to be closed, and it is evident that the battery can be so managed that the accumulating hydrogen shall force the liquid out of contact with the negative plate, if that is desired. Although the zinc is amalgamated in the most perfect manner, yet there is always hydrogen enough attached to the two plates and suspended in the liquid to drive the liquid below the negative plate.

In this battery *everything* is saved. There is no waste-zinc, no waste of mercury as in the ordinary process of amalgamation, and the sulphate of zinc and hydrogen are saved. The practical value of such an arrangement will be appreciated more readily from an example. For instance, suppose that to produce one horse power for twelve hours, 16 lbs. of zinc were consumed. This would require 20 lbs. of sulphuric acid, and would yield nearly 100 cubic feet of hydrogen, and about 40 lbs. of sulphate of zinc.

2.



The battery shown in fig. 2, is upon the same principle, but where its principal use is for the purposes of the hydrogen lamp, the zinc plate H should not be amalgamated.

A, the glass jar to hold the acidulated water.

B, the inner jar and hydrogen receiver.

G, the perforated negative plate.

E, a wire connected with and supporting the zinc plate.

F, a wire connected with the negative plate.

The stop-cock S and its pipe are connected with one pole of the battery through the medium of the wire of the helix surrounding the electro-magnet, shown in fig. 3.

The spring wire P is attached to the plug of the stop-cock, upon turning which, the spring wires are brought into contact and separated, producing the spark which

ignites the jet. The spark from the wires alone is not generally sufficient to ignite the jet, and I have therefore a small electro-magnet concealed within the cover or cap of the jar, and covered well with cement, to protect it from the action of the acid.

A section of the magnet in the cover is shown in fig. 3.

3.

When the battery is first started, to prevent explosion, it is necessary to take the precaution to bend the wires so that the spark may not ignite the jet until the hydrogen comes off nearly pure.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *New varieties of Narcotin.*—Wertheim has discovered that opium contains two new alkaloids homologous with narcotin. Of these the first by the action of potash lime yields methylamin, the second ethylamin, and the third propylamin. The formulas of the three bases are $C_{44}H_{23}NO_{14}$, $C_{46}H_{25}NO_{14}$ and $C_{48}H_{27}NO_{14}$, and the author designates them, methyl-narcotin, ethyl-narcotin and propyl-narcotin. Treated with sulphuric acid and peroxyd of manganese all three bases yield opianic acid and cotarnin; Wertheim considers it highly probable that in this decomposition three different species of cotarnin are produced corresponding to the different narcotins and which may be termed methyl-cotarnin, ethyl-cotarnin and propyl-cotarnin. Wertheim has also found that herring-brine which has the peculiar odor of propylamin actually contains this base in considerable quantity and that the latter may be readily obtained by distilling the brine with a solution of caustic potash.—*Journal für praktische Chemie*, liii, 431.

2. *Equivalent of Phosphorus.*—Schrötter has determined the equivalent of phosphorus by burning amorphous phosphorus in oxygen gas. A mean of 10 determinations which scarcely differ gave as the true equivalent 387·5 on the oxygen, or 31 upon the hydrogen scale. One gramme of phosphorus according to this yields on burning 2·289186 grammes of phosphoric acid. Pelouze's determination—32—is consequently too high.—*Journal für praktische Chemie*, liii, 435.

3. *Production of Cyanid of Potassium.*—Rieken has confirmed by careful experiments the results of Bunsen and Playfair that cyanid of potassium is formed when carbonate of potash intimately mixed with carbon is heated to whiteness in a current of previously heated nitrogen gas. The temperature must be that at which potassium is formed and the nitrogen must be strongly ignited before passing over the mixture. The necessity of fulfilling these conditions will render the process very difficult of execution upon a large scale.—*Ann. der Chemie u. Pharmacie*, lxxix, 77.

4. *Constitution of Human Fat.*—Heintz has published an elaborate and important memoir on the composition of human fat. The conclusions to which he has arrived are stated as follows:

(1.) Human fat does not consist as formerly supposed simply of olein and margarin, but of at least six different fats.

(2.) The first of these fats is contained only in very small proportions, but from an analysis of the acid obtained from it, appears to be identical with the stearophanin discovered by Francis in the berries of cocculus indicus. The constitution and properties of the acid so far as they could be determined, corresponded with those of the stearophanic acid, $C_{36}H_{36}O_4$.

(3.) The second fat is anthropin. The fatty acid obtained from it by saponification is remarkable for its great capacity for crystallization. It separates in broad brilliant crystalline leaves both from the alcoholic solution and when it passes from the liquid to the solid state. Its constitution appears to be represented by the formula $C_{34}H_{32}O_4$, but further investigations must be undertaken to place the correctness of the formula beyond a doubt.

(4.) The third fat is margarin which yields by saponification margaric acid, $C_{34}H_{34}O_4$.

(5.) The fourth is palmitin which yields palmitic acid, $C_{32}H_{32}O_4$.

(6.) Palmitic acid is identical with the acid which is obtained by the action of fused potash upon oleic acid and which Varrentrapp termed olidic acid.

(7.) The fluid portion of human fat consists essentially of olein. It contains however at the same time a small portion of another fat which by saponification yields an acid the baryta salt of which differs in constitution and properties from oleate of baryta.

(8.) The solid substance which separates during the winter from the fluid parts of human fat expressed during the previous winter, contains a considerable quantity of free fatty acid. Human fat must therefore undergo a gradual decomposition by which the glycerine is gradually destroyed and the fatty acid separates, a species of decomposition long since observed in those fats the acids of which are volatile and which plays a part in the changes of butter.—*Pogg. Ann.*, lxxxiv, 260.

W. G.

5. *On Schönbein's Ozone;* by Professor FARADAY, (Proc. Roy. Soc., June 13, 1851.)—The object of the speaker was to give a brief account of the present state of this subject; taking at the same time notice of the ancient facts which belong to it, and the high hopes of progress which it offers for the future. Ozone is produced when the electrical brush passes from a moist wooden point into the atmosphere, and indeed in almost every case of electrical discharge in the air; or when water is electrolyzed, as in the case of a dilute solution of sulphuric acid or sulphate of zinc; or when phosphorus acts at common temperatures on a moist portion of the atmosphere. For the latter case, take a piece of clean phosphorus about half an inch long, which has been recently scraped; put it into a clean two-quart bottle, at a temperature of about 60° F., with as much water as will half cover the phosphorus; close the mouth slightly so that if inflammation take place no harm may happen; and leave it. The formation of ozone will quickly occur, being indicated by the luminous conditon of the phosphorus, and the ascent of a fountain-like column of smoke from

it. In less than a minute the test will show ozone in the air of the bottle, in five or six hours it will be comparatively abundant; and then the phosphorus being removed and the acids formed at the time washed out, the bottle may be closed and made use of when required for experiments.

The test for ozone is as follows: 1 part of pure iodid of potassium, 10 parts of starch, and 200 parts of water are to be boiled together for a few moments. A little of this preparation placed on writing paper with a brush being introduced into the ozone atmosphere is rendered instantly blue from the evolution of iodine:—or if bibulous paper be dipped into this solution, and then dried, it forms Schönbein's Ozonometric test: for a slip being introduced dry into an atmosphere supposed to contain ozone, after remaining there a longer or shorter time, on being removed and then moistened, instantly becomes more or less deeply blue if ozone be present.

Ozone when obtained by the three very different processes described is identical in every respect: its properties are as follows: 1. It is a gaseous body of a very peculiar odor: when concentrated the odor approaches to that of chlorine; when diluted it cannot be distinguished from what is called the electric smell. 2. Atmospheric air strongly charged with it renders respiration difficult, causes unpleasant sensations, and produces catarrhal effects (by acting powerfully on the mucous membranes). Such air soon kills small animals, as mice, when placed in it; so that ozone in its pure state must be highly deleterious to the animal economy. 3. It is insoluble in water. 4. Like chlorine, bromine, and the metallic peroxyds, it is a powerful electrōmotive substance. 5. It discharges vegetable colors with a chlorine-like energy. 6. It converts phosphorus ultimately into phosphoric acid; it combines with chlorine, bromine, and iodine; it does not unite with nitrogen under ordinary circumstances, but does when lime water is present; and nitrate of lime is formed from which nitre may be readily obtained. 7. At common and even low temperatures it acts powerfully upon most metallic bodies, producing the highest degree of oxydation they are capable of. Lead and even silver is carried at once to the state of peroxyds; arsenic and antimony produce arsenic and stibic acids. 8. It transforms many of the lower oxyds into peroxyds; thus, the hydrate of the oxyds of lead, cobalt, nickel, and manganese become in it peroxyds: the basic oxyd of silver undergoes the same change. 9. It decomposes rapidly the solid and dissolved protosalts of manganese; the hydrated peroxyds of the metal being formed, and the acid of the salts evolved. 10. It decomposes the solution of the tribasic acetate of lead; the peroxyd of that metal and the ordinary acetate being formed. 11. It rapidly converts the protosalts of iron and tin into persalts. 12. It destroys many hydrogenated gaseous compounds; the combinations of hydrogen with sulphur, selenium, phosphorus, iodine, arsenic, and antimony are thus affected. It appears to unite chemically with olefiant gas in the manner of chlorine. 13. It instantly transforms the sulphurous and nitrous acids into the sulphuric and nitric acids, and the sulphites and nitrites into sulphates and nitrates. 14. It changes many metallic sulphurets (as those of lead and copper) into sulphates. 15. It decomposes many iodids in their solid and dissolved state. By its continued action iodid of potassium becomes con-

verted into iodate of potassa. 16. It changes both the crystallized and dissolved yellow prussiate of potassa into the red salt, potash being evolved. 17. It produces oxydizing effects upon most organic compounds, causing a variety of chemical changes; thus guaiacum is turned blue by it. From the above enumeration it would appear that ozone is a most ready and powerful oxydizer, and in a great number of cases acts like Thenard's peroxyd of hydrogen, or chlorine, or bromine.

A number of the actions of this body, such as the bleaching of indigo and litmus, the peroxydation of metals, the conversion of sulphurets into sulphates, &c., were shown, to illustrate the chlorine-like action of the ozone; and many illustrations supplied by M. Schönbein himself were exhibited.

With respect to the nature of this body, the two chief ideas are—that it is a compound of oxygen analogous to the peroxyd of hydrogen, or that it is oxygen in an allotropic state, *i. e.*, with the capability of immediate and ready action impressed upon it. When an ozonized atmosphere is made as dry as possible, and then sent through a red hot tube, the ozone disappears, being converted apparently into ordinary oxygen, and no water or any other result is produced. This agrees with the known fact, that heat prevents the formation of ozone, and also with the idea that ozone is only oxygen in an allotropic state. To show that heat prevents the formation of ozone a little voltaic battery was associated with a fine platina wire helix, insulated, and connected with the electrical machine; at first the circuit between the battery and the helix was left incomplete; and then on working the machine the brush thrown off from the helix affected the test paper, before described, by the ozone in it; but when the connection was complete, so that the helix was ignited, then the electrical brush from it had no power of producing any effect of ozone.

The speaker described the presence of ozone in the atmosphere, the mode of testing its presence, and the probable effects it produced there. He referred to Schönbein's recent experiments in the insulation of the oxygen of the air and the peculiar effects produced by this action. He showed by experiments, the more recent results of the association of oxygen by light with oil of turpentine and other bodies; and the production of bleaching compounds vying with the hypochlorite of lime in energy. He made it manifest by experiment, that when ether vapor is mixed with air, and a hot platina wire or glass rod introduced, the ether in becoming partially oxydized to produce acid, also produces ozone, the results bleaching indigo powerfully; and he stated that sulphurous acid, ether, tartaric acid, and many other substances which being first mixed with air or oxygen were then exposed to sunlight, exerted bleaching powers often of a very high degree. The evening concluded with the expression of certain theoretical expectations, or rather possibilities, which were put forth as indicating the probable fertility and importance of the subject, and fitted to excite such philosophers as were engaged in the consideration of the physical qualities of the particles of matter to examine how far the phenomena of ozone might be carried onward in the illustration and extension of their researches.

II. Mineralogy and Geology.

1. *On a locality of Carbonate of Strontian;* by Prof. O. Root.—This interesting mineral has recently been found in the rocks of the Clinton group in Oneida Co., N. Y.; it there occurs in geodes associated with the sulphate of strontian. The carbonate occupies the outer part of the geode and envelops the sulphate; in some instances it forms a white coating spread upon the blue crystals of sulphate, making very pretty cabinet specimens. The sulphate of strontian associated with carbonate of iron, calcareous spar, quartz crystals and sulphate of barytes, has long been known from the rocks of the Clinton and Niagara groups, but the carbonate, although ascribed to these rocks under barystrontianite, has heretofore seldom been found by collectors.

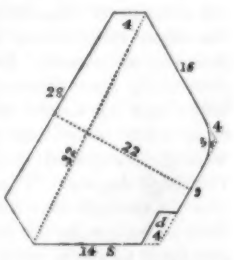
Clinton, N. Y., Jan. 9, 1852.

2. *Beryls in Grafton, N. H.;* by Prof. O. P. Hubbard, (communicated for this Journal.)—The highlands between the Merrimack and the Connecticut rivers contain remarkable granitic veins of great width and miles in length.

At one of these veins in Acworth and Alstead, mica is wrought extensively; moreover the feldspar is coming into use for glazing the stone pottery manufactured at Bennington, and is worth when delivered at the railroad on the Connecticut river, $10 a ton. Mica has also been obtained from a vein in Orange on the west side of the mountain, and largely, as is well known, from a vein in Grafton at Glass Hill, two miles south of Orange summit.

About one mile south of Glass Hill is a locality of beryls in a similar vein composed mostly of quartz and feldspar, which seem to have crystallized on an enormous scale and without presenting regular forms, having very large planes of contact with each other. Two beryls were exposed here last summer that for their dimensions are worthy of notice and surpass in bulk and weight any examples of crystallization that I have met with. I arranged to have the one described below as No. 1, taken from the rock; but the means were quite inadequate, and as the structure of the crystal was unfavorable it was ruined; another season under better auspices an attempt will be made, and I trust successfully, to take out No. 2.

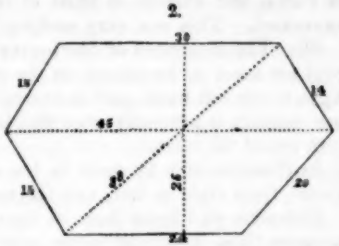
1.



The rock around the beryl, fig. 1, (which lay upon the side S,) had been removed so as to expose all its dimensions. It lay at an angle of 45°, dipping south, and was four feet and three inches long. The rocky mould extended two feet above the base or upper extremity of the crystal, and the diameter was two feet. The crystal was originally six and a quarter feet long. It was quite regular in form, and the upper four feet prismatic and diminishing in diameter one inch in a foot, making the diameter of the base 1·10, the average of the prism. For the remaining two and and a quarter feet, it was distinctly pyramidal, and about four inches diameter at the apex. Some portions of the crystal were very handsome.

It contained subordinate prisms as is usual with these large specimens, and it was seamed transversely here and there with a feldspathic layer.

The other crystal, No. 2, which was near the former, had also been broken off 18 inches, and its superior termination presented the outline and dimensions of fig. 2. The dimensions of both are given in the figures in inches on a scale of one foot to half an inch, and their cubical contents and weight are as follows.



Area of the base of the prism, fig. 1, = 521 square inches = 3·68 square feet; this multiplied by 4 feet, the length of the prism, gives 14·7 cubic feet. Now as 1 cubic foot of water weighs 1000 ounces and the specific gravity of the beryl is 2·675, a cubic foot weighs 2675 ounces, and 14·7 cubic feet will weigh 2445 pounds. The frustum of the pyramid was 27 inches long, width of base 20 inches, of top 4 inches. This gives 434·1 as the area of the base of the frustum, 17·36 as the area of the upper end, and 468 pounds as the weight of the frustum. Hence the weight of the prism is 2445 lbs. + 468 lbs. = 2913 lbs.

The area of the base of the beryl, fig. 2, is 928 square inches = 6·44 square feet. Taking the specific gravity of beryl as before, we have 2675 ounces × 6·44 = 17227 ounces = 1076 pounds as the weight of one foot in length of the crystal.

3. *On Stigmaria*, (Proc. Bost. Soc. Nat. Hist., in Boston Traveller of Jan. 7, 1852.)—Mr. J. E. Teschemacher exhibited a specimen of anthracite coal containing a flattened branch of Stigmaria one foot in length, and three inches in diameter, with the usual markings of cicatrices of foliage, two of which were very perfect.

He remarked that the knowledge hitherto promulgated of this fossil plant, so abundant in all the coal formations, had been chiefly obtained from specimens in the coal measures, but not from those in the coal itself,—and that the opinions of its affinity to the families of plants of the present day varied much. The doubts respecting its nature he thought could only be dispelled by instituting close comparisons, and this he proposed to do between what was already known on the subject, the fossil specimens on the table, and our well known tree, Picea balsamifera, of which he produced fresh specimens:

1st. The cicatrix of the leaf as shown in the perfect specimens on the coal, when examined with a good Coddington lens, agrees minutely with those of Picea, except in size: the fossil being $\frac{1}{8}$th, the recent $\frac{1}{16}$th of an inch in diameter,—the cicatrices of Picea are persistent even on old wood.

2d. The form of the leaf of Stigmaria, as given in figures in the various publications, is linear with an obtuse termination, a midrib, and thickness at the edges produced by involution of the margin:—such is the leaf of Picea.

3d. In the fossil, the leaf is sessile or without petiole, as in Picea.

4th. In Göppert's work, Gatt. Foss. Pfl., 1 and 2, tab. 10, fig. 10, is a figure of a forked branch of fossil Stigmaria, with the cicatrix of a leaf at the angle of the fork; this leaf may be seen in just that position in Picea, and existed in most of the branches Mr. Teschemacher had examined. This is a very striking resemblance.

5th. The cicatrices of the leaves in the specimen on the anthracite coal are seen to be placed on the stem in a spiral direction, from the right to the left hand, and the termination of each spiral, that is where one cicatrix is precisely over that beginning the spiral, is in the eighth turn round the stem.

In Picea as may be seen in the specimen, the arrangement is also spiral, from right to left, and the spire terminates in the eighth turn.

Botanists well know how to appreciate this character, but in order to spare time it will be better to refer merely to the works of Braun, Martins and Henslow on this subject, observing, however, that the number of leaves of the spiral agrees in the fossil with the recent plant, although the cone of Picea, as the most normal exposition of this character, gives twenty-one.

6th. In the figures of Stigmaria by Lindley and Hutton, copied by Göppert, the branches are represented as proceeding in one plane at right angles with the central stem, and at a short distance branching in forks—this as all know is the growth of the whorls of branches of Picea.

7th. The woody structure of the Coniferæ is very characteristic; the central pith is cellular; there are few spiral vessels, but the chief mass is pleurenchyma or ligneous tissue with glandular markings. Mr. Teschemacher exhibited specimens of charred pine wood for the purpose of comparison with charcoal on the anthracite; it was seen that the resemblance, particularly of the annual rings, is striking. Had the lines on the fossil been made by vessels, as in leaves of Gramineæ, the impression of the vessels, or perhaps, portions of the vessels would appear, as in numerous other specimens of leaves. These markings are what Göppert, in his prize essay, calls the annual rings of Araucaria; —the specimen on the table was by far the finest one in the collection of Mr. Teschemacher.

He observed that he could not measure the weight of evidence necessary to produce conviction in the minds of others, but he felt persuaded that the abundant Stigmaria was the fossil of a Coniferous tree nearly allied to Picea. It might be asked why no remains of cones were found. Mr. T. exhibited a specimen which appeared to him more to resemble the impression of a cone than of any Lepidodendron or Sagenaria hitherto figured, but as he had found only two specimens of this nature, he had not at all a decided opinion on the subject; yet even here the spiral direction was from right to left.

With respect to the existence of numerous fossil fungi in the anthracite, he had already exhibited specimens of the rattan from Singapore, (the Palm tribe,) on the epidermis of which were fungi exactly resembling, both in appearance and mode of growth, those on the anthracite. He now desired to add the following observations.

When the globular masses from the coal (resembling sphæria) were exposed to heat sufficient to burn off the carbon and other matters, much peroxyd of iron remained, as was seen in exhibited specimens;

sulphur is found oozing out in most of them, and particularly on those containing fungi; and of all vegetable productions fungi contain by far the largest proportion of nitrogen. Now chemists are aware that if peroxyd of iron, sulphur, and sal-ammoniac which contains nitrogen, are intimately mixed together and heat sufficient to volatilize the nitrogen compound be applied, the result is bisulphid of iron in small brass-colored cubes—precisely such as are seen in these specimens of fossil fungi.

In these specimens of fossil fungi resembling the starfish a few of these cubes may be seen, but in others three or four times this size, the body is a mass of these yellow-colored metallic cubes.

4. *Bulletin of the Geological Society of France.*—The following are extracts from recent numbers of the Bulletin.

Caves of Adelsberg, Magdalena and Planina in the Tyrol.—These caves have been described by Dr. Schmidtl. The cave of Planina in which runs the river Poik, is of vast extent, and seven times deeper than that of Petdelsberg. It contains a lake and several cascades ten feet in height. Dr. Schmidtl had a boat constructed to explore the inner regions, but met with obstacles in the fallen earth that prevented his progress. The water is very deep and the stalactites often hang so low, that it was necessary to lie down in the boat to pass. The masses of fallen debris are often 80 to 100 feet in height; and as the water passes below them, the voyager has to climb over these islands dragging his boat after him. M. Fitzinger has described from these caves seven species of Proteus, specimens of which are exceedingly abundant; and M. Kollar has found and described some eyeless Crustacea. Dr. Schmidtl's description and plans of the caves will be published in the Memoirs of the Vienna Academy.—p. 158, January 13, 1851; *in a letter from M. Boué.*

A Cavern with Bones recently discovered at Lauw (Haut-Rhin), by M. A. Daubrée.—The jurassic limestone of the vicinity of Lauw, pertains to the lower oolite and constitutes a group of low hills situated adjoining the ancient strata of the Vosges. There are several caverns along the banks of the Dollern, and one recently opened is of great extent, with a series of large chambers, connected by narrow passages. From the entrance gallery four transverse galleries pass off, one of which has been followed for ninety yards. Stalactites are abundant; but in the interior there are no animal remains except refuse from animals that now occupy the cave. But at the entrance of the principal cavern, from six to ten yards above the river, there is a yellowish clay, abounding in calcareous concretions, and among them bones, which according to MM. Lereboullet and Schimper belong principally to species of Ursus. There is also the debris of the wolf, fox and wild boar. The bone clay is covered a yard thick with sand containing calcareous concretions and no bones; and it was obvious that the origin of the deposit was anterior to the present epoch.—p. 170, Jan. 13, 1851.

Geodes filled with water, at St. Julien de Valgague; by M. D'Hombres-Firmas.—The inferior formations of the middle oolite of this region contain specular iron, and still more abundantly, the hydrated oxyd of iron, the latter in concretions. In a clay which occurs in these beds, there are occasional nodules usually four to six inches in diameter, and often larger, some of which contain within a fine drusy

crystallization, and others a large quantity of water. On long exposure to the air the latter lose the water; but fresh nodules will sometimes afford three quarts or more. This water when poured out deposits a little calcareous earth and clay, and afterward the tests show the presence of some sulphate of iron.—p. 174, Jan. 13, 1851.

Researches on the origin of the present state of the terrestrial globe, or its crystal-like cleavage; by M. DE HAUSLAB.—This author after discussing the direction of mountains, and of dykes and cleavages among rocks, deduces some general principles with regard to their direction, and then explains his hypothesis that the surface of the globe presents approximately the faces of a great octahedron. In an octahedron there are three axial planes intersecting one another at right angles; and the positions of the circles on the earth's surface which he lays down as the limits of these planes (or their intersection with the surface) are as follows. The *first* circle is that of *Himalaya and Chimborazo*, passing from Cape Finisterre to the Himalaya, Borneo, eastern chain of New Holland, (leaving on its sides a parallel line in Malacca, Java and Sumatra,) to New Zealand, thence to South America near Chimborazo, the chain of Carracas, the Azores to Cape Finisterre. The *second*, passes along the South American coast and the north and south ranges of the Andes, the mountains of Mexico, the Rocky mountains, Behrings' Straits, the eastern Siberian chains, going to the south of Lake Baikal, near Kiatcha, the Altai, Himalaya, the mountains of Bombay in Hindostan, a point in the northeast of Madagascar (where the summits are 12,000 feet high), the mountains of Nieuwefeld, 10,000 feet high, Cape Caffres, Cape Moro de Saint Martha, to Brazil, the rapids of La Plata, Paraguay, Parana, the elevated basin of Titicaca, the Andes, Illimani near Jaen and the defile of Maranova. The *third* circle cuts the two preceding at right angles, and passes by the Alps, the islands of Corsica and Sardinia, along the basin of the Mediterranean, the mountains of Fezzan, Lake Tschan, the Caffre mountains of Nieuwefeld, the Southern Ocean near Kerguelen's Land, the eastern or Blue mountains of New Holland, straits of Behring, Spitzbergen, Scandinavia, Jutland, etc.

These three great circles point out the limits of the faces of the great hypothetical octahedron. Each of the faces may be divided into eight others by means of lines of accidents of minor importance, so as to make in all forty-eight irregular triangles, a form of the diamond. At the intersections, M. de Hauslab observes that there are nodes of dykes, and along the lines or near them, all the mountains of the globe occur. The author gives an extended illustration of his subject and afterwards considers the particular history of the configuration of the earth's surface in accordance with his hypothesis.

M. Boué who adopts similar views adds as a note, that we should remember in this connection that the metals crystallize either in the tesseral or rhombohedral systems, and that native iron, the most common constituent of meteorites, is octahedral in its crystals.—pp. 178–194, Jan. 20, 1851.

M. Boué afterwards states in a letter addressed to M. Viquesnel, that the hypothesis that the surface of the earth may lead to the idea of the globe's being a polyhedral crystal instead of a sphere was brought out

by La Metherie in the Journal de Physique, xvii, 251; xlii, 132; xliii, 355; xlviii, 66; lxxi, 172, 382; lxxviii, 241; lxxxi, 288; and in his *Theory of the earth* in 1795 and his *Lessons on Geology;* also by Oken in his Lehrbuch der Naturphilosophie, 1809, pp. 149, 154; by R. Jameson, Mem. Wern. Nat. Hist. Soc. Edinb., 1814, ii, 221—p. 273, March 17, 1851.

On a Hydrobarometer, a new instrument for ascertaining the depth and temperature of the Sea; by M. H. WALFERDIN.—M. Walferdin shows that the bulb of an ordinary thermometer, formed by being blown on the extremity of a tube, is compressible between the finger and thumb so far as to cause the mercury to ascend appreciably, and that consequently the ordinary arrangement for ascertaining the temperature at depths is faulty. At the artesian well of Grenelle, before the jetting of the waters, six *maxima* thermometers ("thermometers à maxima à déversement" of M. Walferdin*) protected from pressure and sunk to a depth of 505 meters, indicated as the mean temperature 26°·43 C. A seventh thermometer at the same time was sunk, unprotected from the pressure, and it indicated 39°·50 C. A pressure of 50·5 atmospheres had thus increased the result by 13°·07 degrees, equivalent on the arbitrary scale of the instrument to $\frac{1}{4}$° an atmosphere. This experiment was performed by Arago and M. Walferdin.

It hence follows that a *minima* thermometer, protected from the action of pressure, will indicate the true temperature of depths; while a *maxima* inverting thermometer exposed to the pressure, and sunk with the other, may indicate the pressure of the sea. Such is the principle upon which M. Walferdin constructs his new instrument which he calls a *hydrobarometer.*

The best means of avoiding the effects of pressure on a thermometer, according to careful trials, is by enclosing the instrument in tubes of glass, more or less thick according to the pressure they will have to endure, and hermetically sealing the tubes.—p. 214, Feb. 3, 1851.

On Mount Ararat; by M. ABICH.—The great Ararat stands to the southeast of little Ararat; and the two are situated in the longer axis of an elliptic volcanic system, and at the centers of the ellipse. This system occupies a plain, gently inclined to the northeast, which forms the natural slope between the high plain of Bajazed and that of the Araxes, the former 865 yards, the latter 1608 yards in height. The great Ararat has in its upper part the form of a segment of a cone, slightly curved, and truncated towards the northeast, opening towards the Araxes. Ararat viewed on the side of the Araxes is a broad-backed mountain of imposing grandeur owing to its breadth and the wild features of the crater shaped-cavity it encloses; while on the other sides it has the regular form of a pointed cone.

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* M. Walferdin's inverting thermometer is filled to the point with mercury, and the increase of temperature or action of pressure is shown by the amount of mercury which passes the point and is reversed. The instrument, he observes, is easily adjusted with the mercury to the point, at the temperature of the surface of the sea; and after the experiment, it is to be subjected to the same surface temperature, when the quantity of mercury failing, will be read off on the scale. The maxima thermometer in his experiments here mentioned, is used simply for the measurement of pressure, or as a barometric instrument.

The gorge of St. James impresses upon Ararat the appearance of a great crater of elevation. The interior structure of the mountain is here exposed to view, showing trachytic rocks containing pyrites, arranged either in irregular beds or in extensive masses of conglomerates. There are however no modern lavas. These are found along the longitudinal axis of the system, and form two eruptive regions at opposite extremities of the line. That to the *north* includes a conical mountain of regular form, and the whole region (called Kipgoell,) appears to be a product of successive volcanic eruptions. It terminates in a plain having a height of 3248 meters (3552 yards) which is richly covered with vegetation excepting a lava region near its center. Here are two vast crateriform cavities near the place of ejection, looking like deep gulfs of subsidence, and exposing a succession of layers of compact lava beds alternating with beds of scoria. On the *south* side, the volcanic action has opened a large gorge in the flancs of the great Ararat, which may be traced to the top making an elongated niche or gorge which extends downward and enlarges to its base. Below these is a plain like that of Kipgoell, with a similar crateriform pit or gulf. There is a long series of cones of eruption upon the eruptive band which communicates with the aforesaid gorge, several of which are regular cinder or scoria cones and have ejected streams of lava. Other hills of scoria have no craters.

The part of the mountain between the two regions of eruptions is comparatively easy of ascent. M. Abich made his first attempt on the 16th of August, 1844, but was repulsed by a heavy storm. Again on the 23d, he encountered another terrific storm, accompanied with electric discharges of great intensity, and so powerful was the electrical movement that for a long time small phosphorescent flames were seen going from the extremities of several metallic instruments and fluttering from the iron heads of their canes whenever they gave them a vertical position. A fall of snow continued through the night till 10 the next morning and covered the whole cone with sleet to more than a foot in depth, making the route extremely slippery, and that of their ascent thus far nearly impassable. A third attempt was made on the 3d of September; but was unsuccessful on account of the ice.

Again on the 28th July, 1845, M. Abich made his fourth trial, and reached the top at noon on the 29th. The top corresponds to the most elevated part of the west side of a great crater of elevation. This side has the character of a back with a gently rounded and undulated surface, and varied with several low hills, running in a line nearly northeast and southwest. The two middle hills are the proper top of Ararat; the left one was visited by Parrot.

The neck between the great and little Ararat is low and flat and includes a perfectly horizontal plane about 550 yards broad. The debris on the summit of little Ararat consists of fragments of a rock like the Andesite of South America.—p. 265, March 3, 1851.

Water Spouts.—M. Boué describes three water spouts, which he observed, while on a neighboring mountain to *rise from* the surface of Lake Janina. His sketches represent three oblong inverted cones resting on the lake and terminating abruptly above; and he says that the cones or funnels were empty within, as he could look down into

them from the elevated place where he stood. The day was clear and hot, and there were neither clouds nor wind. The particles of water in the cones had a spiral motion from east to west with a linear progress from south to north.—p. 274, March 17, 1851.

Among the many others papers in the Bulletin there are the following: *On the Jurassic, Cretaceous and Nummulitic formations of Bithynia, Galatia and Paphlagonia;* by M. P. DE TCHIHATCHEFF.—pp. 280–312, with a map, March 17, 1851. *Silurian fossils of the vicinity of Rennes;* by M. MARIE RONALT.—pp. 358–389, April 21, 1851.

5. *Memoires de la Société Géologique de France*, 2nd ser. vol., iv. Part 1. 202 pp. 4to, with 11 plates. The first memoir is on the fossils of the secondary period collected in Chili, by J. DOMEYKO, Prof. Chem. de Geol. et de Min. á l'Univ. de Coquimbo, and on the strata to which they belong, by MM. BAYLE and H. COQUAND. The fossiliferous beds of Copiapo are shown to belong to the upper lias and lower oolite. The beds of the Andes of Coquimbo, rise to a height of 5000 metres above the sea. At Porteruelo de Doña Ana, they reach 4094 meters and belong to the middle oolitic period. At Arqueros, the beds contain *Crioceras Duvalii* and *Ostrea Couloni*, species characteristic of the Neocomian formation of Europe. The rocks called Jurassic by these authors were considered Cretaceous by D'Orbigny in his work on South America; this formation appears to be of great extent in Chili and is found also in Peru.

The 2nd memoir (150 pages) is upon the Gneissic formation of La Vendée, by A. RIVIÈRE. The 3d, on a new Pyrenean type parallel with the true chalk, by A. LEYMERIE.

6. *Researches in Terrestrial Physics;* by HENRY HENESSY, M.R.I.A., etc., (Phil. Trans., Part 2, for 1851, pp. 495–547.)—Prof. Henessy in his Researches, has brought to bear the higher mathematics upon various questions connected with the changes in a globe cooling from fusion, its figure, mode of cooling, structure of the crust and nucleus, thickness and fractures of the crust, and other points. We can only in this place call attention to his profound investigations into this branch of terrestrial physics. The author shows in the course of his paper that the solidification of the globe could not have begun at the centre, and on the contrary must have gone on from the surface, adding successive layers of cooled rock. He deduces that the least possible thickness of the crust is 18 miles, and its greatest possible thickness, 600 miles. To arrive at this result, Prof. Henessy employed expressions that he had obtained, "in which the variation of gravity at the earth's surface is a function of the radius and ellipticity of the fluid nucleus supposed to exist within it," thus affording a method of deducing "the limiting values of that radius, and consequently, of the thickness of the solid shell."

The value of the earth's ellipticity when entirely fluid, as obtained by Prof. Henessy, was $\frac{1}{302 \cdot 22} - \frac{1}{316}$; its primitive ellipticity was therefore a little less than its present ellipticity.

The author infers also that a considerable amount of friction and pressure must exist between the shell and fluid nucleus. Still, the highly crystalline structure which must characterize the shell's inner surface, combined with the viscidity of the strata of the nucleus in immediate contact with it, would tend to equalize the motions of both shell and nucleus, and to cause the whole to rotate as one mass.

It is also shown that if the rotation of the earth were originally stable about its axis, it would continue to rotate in the same way forever.

With regard to the lines of elevation on the earth's surface, he observes, that if a zone of least disturbance existed near the parallel of mean pressure, the directions of great lines of elevation should be nearly parallel or perpendicular to the equator. But as yet, observation seems to prove that such a zone does not exist on the earth's surface; and hence, from Section VIII, in his paper, it provisionally follows "that the constant pressure greatly predominated over the variable pressure, and consequently, that the direction of lines of elevation must be comparatively arbitrary. Geological and geographical observations present results which are generally in accordance with these views."

7. *Remarks on the Topography of the State of New York;* by Prof. GUYOT, (from Prof. Guyot's Report on the various Meterological stations established in New York, in the Rep. Regents Univ., 1851, 232.) —The main mass of the state of New York may be said to be a high triangular tract of country, or table-land, elevated from 1,500 to 2,000 feet above the ocean. It may be considered as the northwestern extremity of the plateaux which form in this latitude the western half of the great Apalachian system. The natural limits of this massive belt are, in the west and north, the large depression partly filled with the waters of the great lakes, Erie and Ontario, and which continues its northeastern course down the St. Lawrence to the ocean; in the east the long and deep valley occupied by the Lake Champlain and the Hudson river. But in the south the table-land continues uninterrupted into the state of Pennsylvania. The eastern edge, along the Hudson and Champlain valley, is formed by a series of chains of mountains, more or less isolated from each other, which bear the highest summits in the state, the highlands which cross the Hudson, the Shawangunk mountains and the Catskill on its western banks, and the system of the mountains of Adirondac in the neighborhood of Lake Champlain. Beyond this eastern wall, the true mountain chains cease; but the surface of the western plateau is indented by valleys, the bottom of which is generally several hundred feet below the general level, and which have between them high ridges. A last feature, which is not the least remarkable, is a deep transversal cut, forming the valley of the Mohawk and of Lake Oneida, which opens a way from the low country around Lake Ontario to the Hudson valley, through the whole belt of table-land, and separates it into two distinct masses.

The state is thus naturally divided into four great physical regions to which we must add a fifth, that of the sea shores. They are,

1. The southern, or maritime region.
2. The eastern, or the region of the Hudson river and Champlain valley.
3. The western, or the region of the western plateau.
4. The region of the great lakes, Erie and Ontario.
5. The northern, or the region of the plateau and mountain of Adirondack.

I beg leave to state, in a few words, the general character of each of them, and to indicate the meteorological stations placed therein.

(1.) The *southern or maritime region* contains New York city and its neighborhood, especially Long Island, and may be extended as far as Westchester county, no part of it being scarcely more distant from the sea shore than twenty-five miles. This region, with its flat and sandy beaches, its low grounds surrounded by water, only occasionally varied by inconsiderable hills which never rise higher than several hundred feet, is entirely open to the influence of the sea winds that sweep over it without obstacle. This circumstance, and the southern exposure, give it the highest mean temperature within the state. Six stations belong to this region. Erasmus Hall, at Flatbush, at the western, and East Hampton, at the eastern extremity of Long Island, are believed to manifest the extreme character of the maritime climate of the coast. North Salem, somewhat in the interior, will show, perhaps, the limit of the immediate influence of the sea. The remaining three belong to the city of New York; they are the Deaf and Dumb Institution, Rutgers Institute, and the Free Academy. These three stations being very near each other, and in similar circumstances, it has been thought better that they should observe at different hours, and make together a more complete series of bi-hourly observations.

(2.) The *eastern*, or the region of the Hudson valley, is a long but narrow strip of land on both sides of the Hudson river, stretching from north to south, surrounded first by hills and low table-lands as far up as the gorges where the river crosses the highlands. The valley widens, higher up, in the extensive plains on the eastern side of the river, which are elevated only some hundred feet above tide water. Notwithstanding this low situation, its climate is generally more severe than could have been expected, owing, no doubt, to the cold northern winds which flow from Canada and Labrador, along the open valley of Lake Champlain, as in a natural channel. The stations are Newburgh and Albany, on the western banks of the river, and Hudson, which will probably be transferred to Kinderhook, in the eastern plains.

(3.) The *western* region, or the high table-lands, between the Hudson valley and Lake Erie, is the most extensive. A depression of the surface, and the change of direction of the general slopes, well expressed by the course of the two main branches of the Susquehanna, seem to indicate a natural division of it into two parts of almost equal extent, the eastern or middle table-land, and the western plateau. They are separated by the deep valleys of Cayuga and Seneca lakes, which cut the whole mass almost through from north to south.

The middle plateau, the eastern edge of which is formed by the mountainous country of the highlands, the Shawangunk and the Catskill, has its greatest elevation in the neighborhood of these chains, at the various head waters of the Delaware, where it rises to a mean elevation of 2,000 feet, and on the heights, close along the south bank of the Mohawk valley, at the head waters of the different branches of the Susquehanna. In this latter portion, the passages from the valley of the Mohawk up to the table-land, show still an elevation of 1,400 to 1,500 feet. The general slope inclines toward the southwest, as the direction of the rivers indicate; and the watercourses, the sources of which are the deep valleys of the Mohawk and of the Hudson, seem to avoid flowing into them, and take an opposite course towards the

western waters. But arrested by the rising mass of the western plateau, the Susquehanna and the Delaware turn suddenly back, enter the chains of the Apalachian system, cut them through at several places, and find their winding way to the Atlantic ocean. The exposure of the plateau toward the southwest opens it to the moist southwest winds, and gives it, perhaps, a larger supply of rain.

The western table-tand is characterized by a remarkable swell of land, the mean height of which ranges from 2,000 to 2,500 feet. It attains its greatest elevation in the southwestern part of the state, and beyond the state line in Pennsylvania, and continues, though rapidly declining, along the southern shores of Lake Erie. This is the region of the watershed. From these heights the waters flow down their slopes in every direction, and reach the Atlantic ocean by three different basins, that of the St. Lawrence, by the Genesee river and Lake Erie; that of the Susquehanna by the Chemung river, and that of the Ohio and Mississippi, by the Alleghany river. Unlike the middle table-land, the portion of this western plateau belonging to the state of New York, inclines towards the north, as the course of the Genesee river already indicates. In the south part of it, in Chautauque, Cattaraugus and Alleghany counties, the bottom of the valleys has still an elevation of 1,400 to 1,600 feet above the ocean. Farther north, Erie and Wyoming counties make a lower terrace, of only 1,000 feet elevation, which fall rapidly down to the plains of Buffalo, Rochester and Syracuse, an elevation of from 500 to 600 feet above tide water, and from these, by a last step, to the lower level of Lake Ontario, 235 feet above the ocean. In the southwest, the plateau shows a still more abrupt slope; from the heights where lake Chautauque lies (1,300 feet) it descends 700 feet down to Lake Erie, 565 feet within the short distance of seven miles.

The stations in the middle plateau are distributed as follows: on the higher grounds, along the eastern ridges, Liberty in Sullivan county, at the head of Mongaup valley, and Delhi, Delaware county, in the upper valley of the Delaware. On the heights of the watershed, along the valley of the Mohawk and Erie canal, Cherry Valley, in Otsego county, at the head of the Susquehanna; Hamilton College, at Clinton, Oneida county; Pompey, on the summit of the ridges, above Syracuse. In the valley of the Mohawk, Canajoharie and Utica. In the interior, Oxford, in the Chenango valley; Cortland Academy at Homer; Newark valley, furnished by the Smithsonian Institution, and Ithaca in the deep valley of Cayuga lake.

Most of these places, though in elevated situations, are nevertheless placed in valleys, and surrounded by heights, which situation may have a marked influence, especially on the course of winds. This was unavoidable, the villages, where the Academies are established, lying generally in the fertile bottoms along the watercourses. But it were highly desirable that some stations should be established in a free elevated position, such as that of Pompey. Meredith near Delhi, at the very summit of the watershed between the Delaware and Susquehanna rivers, over 2,000 feet above the sea level, would be such an one. I consider it very fortunate that a good and reliable observer, Samuel A. Law, Esq., is ready to undertake the observations, if furnished with

instruments. I take the liberty of strongly recommending that this should be done. Meredith will be the highest station in the state, and, with Pompey, the most advantageous for ascertaining the course of the winds.

The western plateau being less settled, the stations are less in number. Three only are situated on the high terrace in the south: Elmira, Alfred Academy, and Jamestown. It were desirable to add two others in the middle part, for instance on the heights of Springville, and at Geneseo in the valley of same name.

(4.) The *region of the great lakes* is reduced, along Lake Erie 565 feet above the ocean, to a narrow strip along its banks; but from the eastern extremity of this lake, it becomes a broad and fertile plain, elevated from 400 to 600 feet above tide water, and 150 to 300 feet above Lake Ontario. It extends itself from Buffalo and Rochester to Lake Oneida. In the western half, this terrace falls by an abrupt and rapid step, marked by the falls of Niagara, down to Lake Ontario, 235 feet above the ocean; and farther east by gentle slopes. It is in this region that the meterological influence of the lakes is more particularly felt. Seven stations belong to it. Fredonia and Buffalo on Lake Erie, Rochester, Geneva College, Seneca Falls and Syracuse in the plain, and Mexico near Lake Ontario. A station was established at Lewiston on the Niagara, near the lake; but the Academy having been broken up, the station has been discontinued after nine months existence.

(5.) The *northern region* is a large tract of country isolated all around by the valleys of Lake Ontario and the St. Lawrence, of Lake Champlain and the Hudson and of the Mohawk. The mean elevation of its central part is from 1,500 to 1,700 feet. It rises rapidly from the Mohawk valley, from Lake Champlain and Lake Ontario, but by very gradual and gentle slopes from the St. Lawrence. Like the table-land south of the Mohawk, the eastern portion is mountainous. Five or six chains nearly parallel, run from S.S.W. to N.N.E., and fill the whole space, 50 miles wide, between Lake George and Lake Champlain and Longlake. This is the group of the mountains of Adirondac, which terminates abruptly in the parallel of Plattsburgh. They are the highest mountains in the state, many of their peaks rising over 5,000 feet above the ocean; they intercept numerous valleys, which are partly filled by a great number of lakes, and give rise to the sources of the Hudson and of various other streams. The western part is a high table-land, much more regular and less indented than that south of the Mohawk. One great feature only is to be remarked, that is the large and deep valley of the Black river, the flat bottom of which is 700 to 800 feet below the general level, and as much above the ocean. The country between the Black river and Lake Ontario, the long slopes descending towards the St. Lawrence, the shores of Lake Champlain, and Lake George and the Hudson are settling and clearing rapidly, but the whole central tract is still a wild almost unbroken primitive forest, interspersed with only a few settlements.

In these circumstances it was not to be expected that a station could have been established in the wilderness, in the almost Alpine region of the high plateau, however interesting it would be to fill up in part, at least, this vast meteorological lacune; but thanks to the liberality

of Hon. Archibald McIntyre, who furnished at his own expense, the necessary instruments, a post was established at the iron works of Adirondac village, in the very midst of the highest mountains of the group, at the request of the Smithsonian Institution and Dr. T. R. Beck. The other places of observation of the lower country are Glens Falls, on the Hudson, Plattsburgh on Lake Champlain, Malone and Ogdensburgh in the northern plains of the St. Lawrence, Lowville in the Black river valley; Boonville on the watershed between this valley and that of the Mohawk. A station is wanted in the southern part of the table-land in the region of Lake Pleasant, in Hamilton County, but no observer could be found there.

The total number of the above-named stations in the state is thus thirty-eight.

8. *Reports on the Albert Coal Mine;* by C. T. Jackson, and Dr. J. G. Percival, with a microscopic examination by Dr. John Bacon, and chemical examinations by Dr. A. A. Hayes, J. R. Chilton, Dr. Ure of London, Dr. John Torrey, Prof. James C. Booth, and others. 48 pp. 8vo. New York, 1851.—The coal of the Albert mine, New Brunswick, has been called asphaltum. It resembles asphaltum closely in lustre, color and compactness, but differs in chemical characters, and in this respect agrees with a highly bituminous coal. It has been supposed to occupy a vein and this has been one ground for considering it distinct from true coal. The investigations detailed in this pamphlet bear upon each of these points.

Dr. Jackson in his report describes the geological relations of the coal and its chemical characters, and the fossil fish and other fossils of the associated beds. He describes the beds as very much folded and contorted, so as to be vertical in many places; and the coal as included between layers running in accordance with the general stratification of the country. In most cases the rock enclosing the coal is much crushed or broken. Figures of natural sections are given illustrating the remarkable contortions described. The coal, unlike asphaltum, does not fuse when heated to 700 F., but burns like cannel-coal; it does not dissolve in naphtha or benzole, only 14 p. c. being taken up by the former and 20 by the latter. The fossil fish belong to the carboniferous period. The species named are *Palæoniscus Alberti, P. Brownii, P. Cairnsii;* and others are indicated. The fossil plants are true carboniferous species; one is near the *Lepidodendron gracile* of Brongniart (ii, pl. 15). Dr. Jackson's Report extends over 28 pages.

Dr. John Bacon, after grinding down thin slices of the supposed asphaltum, detected with a microscope vegetable fibrous tissues much contorted, enclosing cells and penetrated by numerous apertures approaching a circular form which appear to be transverse sections of vessels; but the tissues were too much broken to enable him to form a positive opinion in regard to the nature of the plants to which they belonged, though abundantly showing that the material was true coal and not asphaltum.

Dr. Percival was confined to a gelogical examination of the region, and arrived at the conclusion that the coal occupied the position of a true bed. His observations are important, but we can give at this time only his results, which agree with those of Dr. Jackson:

"1st. The bed in question is situated in a bituminous shale, which, from its own fossils, and from its connection with rocks, having the fossils and other characteristics of the coal formation, belongs itself to that formation.

2d. The shale, for a considerable distance around the mine, exhibits great irregularities and contortions, caused by disturbances which have changed its dip from horizontal to nearly vertical.

3d. The principal portion of the mine lies, on the whole, in a direction between the strata, and presents on the surfaces of its walls, appearances of deposition in a soft aqueous condition, and not of a rupture and injection when the rock was indurated.

4th. The irregularities in the mine, even those at the fault, and in the north-east extremity, correspond with irregularities observed in remote parts of the shale, and may be explained by the disturbance necessarily arising from the change from a horizontal to a nearly vertical position of the strata, and perhaps from a contortion in their general direction.

5th. The substance is analogous to cannel-coal and jet, in which the original lamination is nearly or quite obliterated, and which, like all substances which have hardened from a liquid or very soft state, are divided by jointed seams, conforming, in their arrangement, to the bounding walls; and, conformably to this, the jointed seams or divisional planes of the bed, instead of being always horizontal, and unconformable to the walls, are, in every point examined by me, strictly conformable to them.

6th. This substance, when tested by the flame of a candle, and by red-hot iron, or iron heated just below ignition, exhibits the characters of coal, and not those of asphaltum."

Dr. A. A. Hayes compares the chemical characters of asphaltum and the Albert mine coal, and points out the many dissimilarities between them. We here cite a few of those mentioned.

Coal of Albert mine.	Asphaltum.
Specific gravity at 60° Fah., 1·0836–1·1113.	Cuba asphaltum, specific gravity 1·165–1·170.
Powder, black.	Powder of asphaltum, brown.
Does not fuse at 700°.	Fuses completely below 220°.
Less than 10 p. c. dissolved by oil of turpentine at 212° F.	Wholly soluble in oil of turpentine at 212°.
Hardly acted upon by linseed oil.	Wholly soluble in linseed oil.

Dr. J. R. Chilton obtained in an analysis,

Fixed carbon 40·86, volatile matter 58·48, ashes 0·66=100. This is near the result of Dr. C. T. Jackson, who found in different samples,

	Vol. matter convertible into gas and removed by heat.	Coke.
1.	58·8	41·2
2.	61·67	38·33

Dr. Frederick Penney of Glasgow, obtained on analysis, volatile matter 61·0, pure coke 38·5, ash 0·5=100·0.

Prof. J. C. Booth found, matter vol. at a red heat 59·75, fixed carbon 38·25, ash 0·25, moisture 1·75, and he observes that it has a similar composition to some English bituminous coals.

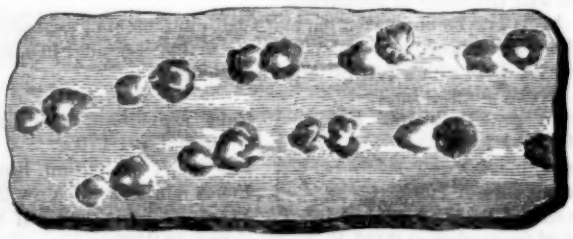
This coal is an exceedingly valuable material for the manufacture of gas for illumination.

9. *Notice of the Remains of Reptiles in the Old Red or Devonian formation of Scotland;* by Capt. L. Brickenden,* F.G.S., and Gideon Algernon Mantell, Esq., LL.D., F.R.S., &c. (An abstract from the Proceedings of the Geological Society of London, December 17th and January 7th.)—Considerable interest has been excited among British geologists by the announcement of the occurrence of the remains of two or more orders of reptiles in the old red sandstone of Scotland, inasmuch as no vestiges of animals of a higher class than fishes had hitherto been observed in the Devonian formation in any part of the world.

The communication by Capt. Brickendon, with descriptions of the fossils by Dr. Mantell, read before the Geological Society of London, gives the following particulars respecting a discovery which is of the highest interest in a palæontological point of view. The old red sandstone strata are largely developed along the coast of Morayshire, and the yellowish crystalline sandstone is extensively quarried in several localities near Elgin, Spynie, &c. These strata, though for years diligently explored by several competent local observers, had yielded but a solitary specimen of organic remains, viz.: the impressions of a series of large scales of a new genus of ganoid fishes, which M. Agassiz named *Stagonolopis Robertsoni*, and has figured in his fishes of the Devonian system.

In the summer of 1850, Capt. Brickendon obtained from a quarry of the yellow sandstone at Cummingston near Elgin a slab bearing the distinct imprints of Chelonian footsteps.† These are thirty-four in

1.



Chelonian footprints in Old Red Sandstone; ¼th natural size.

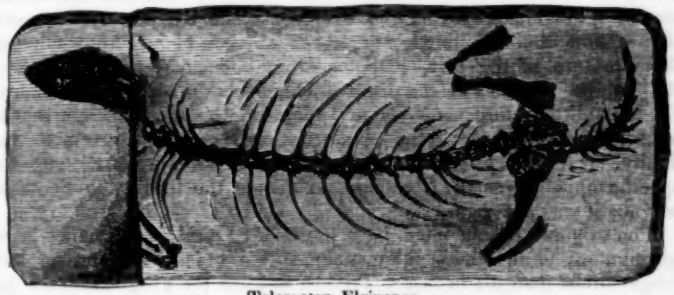
number and extend several feet across the stone. The impressions of the right feet alternate with those of the left, from which they are separated laterally by an interval of three inches, the length of each pace or stride being about four inches. The imprints of the fore and hind feet are nearly in contact; the size of the former in relation to the latter is as three to four; the hinder prints are an inch in diameter. The foot-tracks are obtuse and rounded, and indicate a close connection of the articulations, for no distinct markings of the joints are shown. The discovery of these foot-prints, which in every respect re-

* The name of Capt. Brickendon will be familiar to our readers who have read Dr. Mantell's memoir on the jaw of the Iguanodon, the first specimen of which was found by Capt. B.

† The cuts here given are from the new edition of Lyell's Manual, just published.

semble those from the Triassic and other rocks that are ascribed by palæontologists to turtles or tortoises, is alone an important fact, since it demonstrates the probability, if not certainty, that reptiles existed during the Devonian epoch; hence the attention of those collectors who were aware of the discovery was especially directed to the rocks of Morayshire in the hope of obtaining other vestiges of reptilians, but without success, till in November, 1851, Mr. Patrick Duff of Elgin, the author of an excellent work on the geology of the district, procured from the sandstone at Spynie the extraordinary fossil which forms the principal subject of Dr. Mantell's supplement to Capt. Brickendon's memoir.

2.



Telerpeton Elginense.

This fossil consists of the impression of a great part of the skeleton of a four-footed reptile, about six inches long, in a block of crystalline sandstone which is broken into three pieces. Fortunately the stone is split in a direction parallel with the plane of the spinal column, so that one piece exposes the imprints of the vertebræ and ribs and hinder extremities, and the other the corresponding mould of the upper part. Dr. Mantell by a careful investigation has been enabled to give restored figures of the vertebræ, ribs, femora, etc.; but the original contains no remains of osseous substance, except of the cranium, and that part of the skeleton is crushed and in great measure concealed by the investing stone. The skull appears to have been of an oval form, resembling that of a small lizard or aquatic salamander; but the true outline cannot be determined. There are remains of two or three very minute and smooth conical teeth, but their mode of implantation in the jaws is not obvious. The spinal column from the occiput to the pelvis appears to have consisted of twenty-four vertebræ, each vertebra having a pair of very slender ribs; these processes are short in the anterior and posterior part of the column, and relatively long in the middle dorsal region. The bodies of the vertebræ were apparently doubly concave; their zygapophyses are nearly horizontal; the neural arch forms a slightly elevated dome with an abbreviated spinal process as in the salamanders. There are no remains of the scapular arch, but there are imprints of the left humerus, radius, and ulna; and of the right and left femur, tibia, and fibula. An impression of a subquadrangular form indicates the general shape of the pelvis; there are ten or twelve caudal vertebræ exposed, the remainder of the series being

buried in the stone; the length of the tail probably did not exceed an inch and a half. There are no vestiges of the feet exposed, but possibly careful chiselling might reveal the outline of the hind feet, which seem to pass off at right angles from the distal end of the tibia, into the stone.

Dr. Mantell gives minute anatomical details (obtained by a careful inspection of casts made by pressing a soft substance into the sharpest imprints) with the view of directing attention to the most important characters should detached bones or a skeleton be hereafter discovered. Dr. M. concludes that the original was a peculiar type of air-breathing oviparous quadruped, presenting in its osteology certain characters that are found in Lacertians, combined with others that occur in the skeletons of Batrachians. From the evidence afforded by a mere impression of part of the skeleton, and in the absence of any knowledge of the structure of the feet, scapular arch, bones of the cranium, &c., the natural affinities of the original cannot be precisely determined. Dr. Mantell therefore proposes to distinguish this most ancient reptile hitherto discovered, by a name simply expressive of its remote antiquity, viz.: *Telerpeton*, (a τηλε, *procul*, et ερπετον, *reptilis*,) with the specific appellatives of *Elginense*, to denote the locality in Scotland whence it was obtained.

The original reptile must have borne a general resemblance in its physignomy to an aquatic salamander, with a broad dorsal region, and longer limbs than the ordinary Tritons, fit alike for progression on the land or through the water; the tail appears to have been wide. The entire length of the animal did not exceed six or seven inches.

Fossil ova, probably of Batrachians.—In connection with these discoveries Dr. Mantell brought under the consideration of the Society certain fossils which abound in the lower Devonian shales of Forfarshire, and are figured and described by Sir Charles Lyell in his Elements of Geology, as probably the ova of gasteropodous mollusca. These are clusters of small roundish carbonized bodies, which generally occur with remains of aquatic plants. With the exception of the *Cephalaspis* and other ganoid fishes peculiar to the old red, no other fossils but the ova have been found in these beds. The resemblance of these organic remains to the carbonized spawn of recent frogs which Dr. M. had found in the inspissated mud of a dried up pond, led him to suspect they might be the fossil ova of Batrachians; and for reasons detailed at length in the memoir, (and the author's conjecture is corroborated by Mr. Newport the eminent British physiologist whose experiments on the development of the ova in Amphibia have just been rewarded by the medal of the Royal Society,) Dr. M. is of opinion that if the fossil bodies of Forfarshire be of animal origin (as is supposed) they are unquestionably referable to Batrachians allied to the *Ranidæ* or frog family, and not to Gasteropoda; larger ova occur singly or in pairs, and often attached to the foliage (in like manner as the eggs of our Tritons) and these in all probability belong to aquatic salamanders. In confirmation of his conjecture, Dr. M. particularly dwelt on the fact that in the numberless strata of shales, limestones, clays, &c., abounding in shells often in a state of great perfection, with the ligament, epidermis, and even the soft parts preserved in the state

of molluskite, no ova had ever been detected; while in the shales of Forfarshire which swarm with these eggs, neither shells, nor even casts of shells, had been discovered. An important geological inference was supported by Dr. Mantell as deducible by these facts. Here we have ova with aquatic plants (apparently fluviatile) and the remains of ganoid fishes which for aught we know to the contrary may like the recent Lepidostei and Palypteri have been inhabitants of rivers; and with no vestiges of shells or other marine organisms; phenomena which lead us to inquire whether the lower Devonian strata of Forfarshire may not be of fluviatile or fluvio-marine origin:—whether, in fact we have not here indications of the intercalation of a series of freshwater deposits in the marine formation called the old red sandstone of Scotland? future observations will determine the solidity of this novel but highly probable suggestion. Should this view be established by future discoveries, Dr. Mantell will have had the singular good fortune to be the first geologist who predicated the presence of freshwater deposits in the palæozoic formations of Scotland, as he established by his own researches the fluviatile character of his native Wealden of the southeast of England.

III. Zoology.

1. *The Relations of Embryology and Spermatology to some of the fundamental doctrines of Physiological Science;* by Dr. W. I. Burnett, of Boston, (From the Proc. of the Amer. Assoc., Albany meeting, 1851, and communicated for this Journal by the author.)—When the influence of the study of organic chemistry and microscopy was beginning to be felt in natural science, the prospect was held out that soon we should understand fully the intimate and primordial relations that exist between the organic and inorganic world.

This was promising too much, and a disappointment has necessarily ensued; still, the quite thorough prosecution of these studies has, I think, produced two distinct results or opinions. With the chemists, the tendency has been to regard the movements of the organic world as simply the results of modified chemical forces. In other words, that matter and chemical power include the phenomena of life. With the microscopists, on the other hand, the tendency has been to consider organic matter as endowed with a power above and beyond these others, and that we are to recognize, in the expressions of life, *vital* as well as chemical forces. I believe this is the tenor of all carefully pursued microscopical studies. With chemists, vitality is always materialized; with microscopists, it exists as an entity above and beyond matter, and may be considered as its thought or idea.

There is a great difficulty in investigating subjects of this kind, because we are in constant want of requisite data from which we can safely draw conclusions. But since my attention has been called to the microscopical study of developing forms, or, in a word, to organic atoms, I have been induced to adopt the opinion that, beyond and isolated from matter, there exist not only what is termed a vital force, but ideas and thoughts.

I propose to illustrate these views by a consideration of some results at which I have arrived in a new and rather peculiar department of microscopy—viz.: spermatology, or that which relates to the intimate nature of the spermatic particles. But that these may be better understood, it is necessary that I should refer for a moment to the present conditions and relations of this branch of science.

In embryological studies, we commence with the simple ovarian cell, or even still further back, with its nucleus. This we trace upward until it has grown to a perfect cell. We then watch the endogenous formation of the cells within it, until it is a great compound cell, which is called the ovum; and then we observe the modification of its contents into a symmetrically-shaped body, which is the new being. Now, throughout the animal kingdom, not only has this primitive ovarian cell the same material aspect, but the same is true of the great compound cell or ovum. This, then, is the fundamental point in all development —and from the most careful examination with the highest and best microscopical instruments, we are unable to perceive why one cell should give rise to a spider, while another, appearing exactly like it, should give rise to a bird. We can reason only from what we know, and if in studying the ultimate atoms of two different portions of matter, we can detect no difference with our present instruments, we certainly have a right to affirm that these portions of matter are identical in physical character. We have a right to infer, also, that that power which prompts each cell to its ulterior condition, viz.: the production of a spider or a bird, has, in the cell, no material expression by which it can be determined, but that it resides as a simple, pure force or individual entity.

Some may call it a dynamic power inherent in the cell, but this is only expressing the same in different terms. We have, then, in a single cell, the complete idea of a bird existing not as a material condition, but as a pure individuality; which last is shown by the fact that the idea is not that of simply *a* bird, but *one of* distinct characters, which here exist *in* thought or type so minutely as to comprise even the color of the bill and length of the feathers.

This view is well supported by all facts of hybridization; for when allied species unite, and there is in the offspring a union of the characteristics of each, this last could have occurred only at the time of fecundation, when the ovum was merely a compound cell, and when it must have possessed all the specific character of the female. In this relation I cannot do better than to quote the words of the profound Müller. He says: "The simple embryo, which consists of a granular shapeless substance, is to be regarded as the potential whole of the future animal, supplied with the essential and specific force of the future animal itself." It may be urged that this idea is quite indistinct, and that we are deviating from the true method of physical investigation; by affirming that the forces of a bird, for instance, exist as such before the material organs by which they are to be expressed have been formed; but this objection is not true or valid, not only because it is based on a mere opinion of the nature of organic matter, but also because it is contrary to many facts; for we do have *attempts* at the expression of individual forces long before the organs by which they ulti-

mately find their complete exhibition are developed, or even when they are never developed. Let me therefore add that if careful embryological studies teach anything in this connection, it is that each healthy ovarian cell contains the potential whole of an individual like the parent, and which is constantly seeking its complete development to be attained only by the co-operation of a corresponding element of the opposite sex, and that the type of form ultimately expressed, being the outward exhibition of inward forces, cannot long suffer deviation without destruction, and is fully as permanent as the individual itself.

If we are not allowed these forces in organic forms, we certainly never can rise above the immaterial forms; and there would appear to be no reason why the ovarian cell of a bird should not produce a mammal as well as a bird. But these relations are not equally clearly understood by all; and some whose attainments should have led them to think differently, have so failed in their appreciation of them that they have regarded the ovum as simply an organic molecule, thus putting an end to all dispute in their own minds, as to spontaneous generation and epigenesis.

I have thought, if anything was wanting in embryology to render such views complete, the complement could be fully found in its counterpart science, spermatology, and I have therefore taken it up in that connection.

What embryology is to the female, that spermatology is to the male. In a histological point of view, the process is the same in each sex. In the one, you have a simple cell passing on in development to a new being; in the other, you have a simple cell passing on to the development of an organic vitalizing particle, which is the prototype of that being. There is, however, this difference, which should be remembered—it is, that in embryology, the new individual form is the result of the co-operation of the two sexes; while in spermatology it is the result of one. Therefore, our philosophical studies in the latter begin for the most part where in the former they have ended; for the spermatic particle is the material expression of the male, both generally and specially. It is, in fact, the male embryo. The course of study by which this important truth has been ascertained, I have treated of in another place.

We here begin with the simple testicular cell, the growth of which we watch until it has become the great compound mass, the parent sperm-cell, in which, by a modification of its contents, are developed the spermatic particles. These organic particles are the true and only fertilizing agents in the process of fecundation, in which experience has shown that they do not merely fertilize—that is, light up a pile before already to burn—but they co-operate and furnish conditions essential to the perfect result. We know very well, that, in the higher animals, where often there is a dissimilarity of form and external appearance of the two sexes, the offspring not unfrequently have all the characteristics of the male, even down to minute points.

In our own species, we are daily observing how correctly and faithfully the child often inherits, not only the physical, but also the peculiar mental features of its father; and too often, too, do we see in the same inheritance a variety of disease. Now, experiment has shown that, in this process, a single spermatic particle only is required, and that there

is no incorporation of its substance with the ovum, but that the whole is accomplished by mere contact with the periphery of the ovum. We have, then, in a word, the organic particle of a male, microscopically minute, which, by mere contact, and without any material loss, transmits to the ovum not only the potential whole of the male generally, but minute mental and physical characteristics. It must be, therefore, that in a minute particle of matter there are hidden often not only the silent thoughts or prototypes of poetry and art, but also of peculiar love and affection.

It cannot be otherwise, and is no more difficult to be comprehended, nor is the idea less beautiful in suggestion, than that these same *thoughts*, when matured in after life, with daily expression, should occupy the strange relation which they do with the material substance of the brain.

It is argued that the idea of *transmission* of force is quite unphysical. Because says the chemists, that only which is substantial can be communicated. But this I consider simply reasoning on the ground that a condition in nature is impossible because it eludes our means of study and observation. But if we are led to take views like these, we must not at the same time suppose that all spermatic particles are, like ova, identical in physical appearance; for the spermatic particle is not the analogue of the ovum, but of the new being; and, as in the latter, so in the former we find well-marked differences of type. Still, the physical characteristics of this particle are not always expressive in any way of the individual of which it is the prototype, and both the similarities and dissimilarities afford a fine argument on the point we are now discussing. For instance, I can show a spermatic particle of a duck, which, as to external appearance, as well as by microscopical measurement as to length, breadth and thickness, cannot be distinguished in any way from another of a reptile. Yet in the one is embodied the idea of a bird, even to the color of its plumage; while in the other is embodied the thought of a reptile.

But there is another point from which I wish to view this subject, and which takes us still further back in histology. I refer to the type of the spermatic particle itself. In any animal, there is but one form of this minute particle, and from it there is never any variation, any more than there is in the elimination of the embryo from the female; and exactly as there resides in the ovum the thought or idea of a future embryo of a certain shape, so it may be considered there exists in the sperm cell the thought or idea of a spermatic particle of a peculiar form. This may be safely inferred from the uniformity of results with which we meet; but in the course of my studies in this direction, I have met with phenomena which illustrate this point in a striking and beautiful manner, and at the same time exemplify to us how wondrous and how certain is this type-power in the ultimate attainment of its object.

With one or two preliminary remarks, I will show what I here mean. In the development of a spermatic particle, you have, as I have before said, the parent sperm cell, which is a large cell filled with small ones, each of which is nucleated. The direct formation of the spermatic particles here begins to take place, and it occurs in two general modes. The first is called the special cell; the second the fascicular mode of

development. But in some of the Rodentia, I have observed, even in the same animals, *both* modes of development. In fact, two parent sperm-cells, side by side—in one of which the spermatic particles are eliminated by the fascicular, while in the other by the special-cell mode. The results were exactly the same, and the particles of each could not be distinguished from each other. We have here, then, a single identical result from two dissimilar processes. This fact stands for a great deal; for it would appear that whether the development takes the right or the left hand road, the type-power brings out always the same result. It is destined to get its expression some way, and certainly appears to argue that there are dynamics above and beyond material forms.

When an artist portrays in sculpture the outward forms of the beautiful human face, we can have no doubt that the image of the same lived in his imagination before the material work was commenced. But when the same artist portrays in painting also the same beautiful face, with exactly the same lineaments, this affords an additional evidence that the conception was pure and distinct, and lived as such in his mind; and it mattered but little whether it took this or that outward visible form, for the expression of the creative thought would always be the same.

Exactly so it is with spermatic and embryonic typical forms. The idea or thought behind each proves its individuality by the uniformity of its expression. It matters but little or nothing whether gained in this or that manner.

In conclusion, let me say that I consider a thorough appreciation of these doctrines of what may be termed higher dynamics quite essential to the progress of physiological science; and I cannot see why men should retard it by a series of detailed explanations which really obscure the matter. Why not recur at once, for instance, to vital force, for the explanation of certain phenomena, and then we shall have a point of departure, the determination of the laws of this *vital force* remaining for our future study.

Liebig says: "As soon as physiologists meet with the mysterious vital force in any phenomenon, they renounce their senses and faculties; the eye, the understanding, the judgment, the reflecting faculties, all are paralyzed, as soon as a phenomenon is declared incomprehensible." I do not consider this true, or even if it is partially so, it applies to the chemist as well as to the physiologist. It is useless to cavil about these matters. We must have terms to express phenomena. In the natural world, one thing appears almost as mysterious as another. Even if we do reduce everything to mechanics and chemical action, is the matter then made more clear? Do we know anything about the fundamental principles of mechanics or chemistry? or is a *vital force* more mysterious than is *motion* with the one, or *affinity* with the other? Is it more comprehensible that an elective affinity should exist in inorganic particles than that animal types should exist in organic ones? Of all these matters, we know nothing except from their objective phenomena, and we cannot indeed do less than to show a consistency in their recognition.

2. *An Improved Method of killing and preparing Lepidopterous Insects for Cabinet Specimens;* by J. P. KIRTLAND, M.D.—In making collections of Lepidoptera, the means usually employed for destroying life —such as compressing the thorax, puncturing the thorax with a needle or pin wet with oxalic acid, smothering with ether, chloroform, or fumes of sulphur, are objectionable, on account of their impairing the beauty if not actually mutilating the specimens. A more successful method is to puncture the thorax once or twice with a needle wet with a strong solution of cyanid of potassa, holding the specimen between the thumb and forefinger so as to keep the wings from fluttering. This solution should be kept in a well stoppered phial; and the needles (of different sizes) may be mounted in light handles. To prevent oily exudations and preserve the colors of the body, the contents of the abdomen (if the animal be large, or even of medium size) should be removed and the cavity filled with cotton. For this purpose a common dissecting case containing a delicate pair of sharp-pointed scissors, scalpels, sewing needles, &c., will furnish all the necessary tools except a steel fork. This fork may be expeditiously formed from a small sized netting-needle, by cutting off with a file the blunt end near its extremity, so that the two sides of the eye may form the tines of the fork, each of which should be filed down to the required form and size. The needle may then be shortened at the other end and set in a handle when it is ready for use. This instrument is very useful in all kinds of Taxerdermy.

The specimen to be prepared should be firmly secured on its back by inserting a pin through its thorax into a pine-table. An incision should then be made the whole length of the abdomen with a pair of sharp-pointed scissors. The sides of the incision may be kept separated by inserting between them a pair of elastic dissecting forceps and permitting the blades to expand apart. The viscera can then be scooped out by aid of the hasp of an ivory-handled scalpel gently carried from the thorax, backwards—taking care not to stain, mutilate or discolor the integuments. Pulverized arsenic (arsenious acid) should then be applied liberally to the internal surfaces. The handle of a scalpel also answers the purpose of a spatula for this operation.

To restore the natural form to the body, roll a pledget of cotton into a conical form, and of suitable size, on the end of the fork-tines, and insert it into the body of the insect, the apex of the cone towards the vent. The elastic forceps will still be of use in keeping the incision open while applying the arsenic and inserting the cotton-body. Should not all the parts be filled out to their proper size, small portions of cotton may be carefully crowded in, on the fork, till the work is effectually accomplished.

A little experience in forming the body, on the fork, and in moulding it to its proper size and form by rotating the handle of the instrument in the right hand and compressing the cotton with the fingers of the left hand, will greatly aid in perfecting the operation.

The sides of the incision may now be brought exactly together, so as to show no deformity. Arm each end of a piece of fine thread, a foot in length with a needle, and in sewing, carry the needles from within the incision, out; drawing both sides at the same time.

The specimen should then be mounted in a natural position, usually with the wings extended. For this purpose, provide a strip of pine board or other soft wood, five or six inches wide, and one inch thick, grooved its whole length in the middle of one of its flat surfaces, the groove to be $\frac{1}{2}$ an inch deep and from $\frac{1}{4}$ to $\frac{1}{2}$ an inch in width, according to the size of the insect. It is well to have on hand a supply of different widths.

The pin should be withdrawn and put in from above, fixing the body in the groove of the board, and its point in the wood; the wings may then be expanded to their proper position and fastened by a few small pins. Brackets of card-paper may occasionally be employed, but they are liable to compress and disfigure the delicate pile on the wings.

It is perhaps proper to add that the utmost care is necessary in employing a poison as deadly and virulent as cyanid of potassa. Other classes of insects may be killed with it, but its impressions are more instantaneous on Lepidoptera than on any other. Is it hence to be inferred that their nervous systems are of higher order and that they therefore occupy a higher scale in the order of creation than other classes of insects? This we believe would be in accordance with the views of Prof. Agassiz.

3. *On the Perforation of Rocks by Pholades;* by M. F. Caillaud, (Comptes Rendus, Nov. 24, 1851, 572.)—M. F. Caillaud sustains the view that the rocks occupied by the Pholades is mechanically excavated by the animal, by means of its shell. He observes that in less than an hour and a half, he dug out with one of the shells a hole eighteen millimeters deep in limestone; and shows by the circular scratches in some of the holes, that the animal actually uses a rotary movement in the excavation. Moreover, they work out holes not only in limestone, but also in gneiss, mica slate and other rocks, many specimens of which he had collected; and he denies that an acidulated secretion, (to which such perforations are usually attributed,) can perforate both limestones and gneiss or mica schist.

IV. Astronomy.

1. *Supposed new Planet.*—On the 8th December, 1851, Prof. De Gasparis in Naples, discovered near Saturn a very faint star, which he considered a new planet; but no intelligence of the confirmation of this discovery has yet reached us.

2. *Symbolical Notation of the Asteroidal Planets,* (Gould's Astron. Jour., No. 34.)—On account of the inconvenience resulting from the present arbitrary symbols for the large family of small planets between Mars and Jupiter, it has been agreed upon by several astronomers in Germany, France, England and America, to propose for adoption a more simple system for this group, viz.: a circle containing the number of the planet in the order of its discovery. It is to be presumed that this plan will be generally adopted. The following table gives the names of such of these planets as have hitherto been observed, with the time of their discovery, and the name of the discoverer.

Names.	Date.	Discoverer.
1. Ceres,	1801, January 1,	Piazzi.
2. Pallas,	1802, March 28,	Olbers.
3. Juno,	1804, September 1,	Harding.
4. Vesta,	1807, March 29,	Olbers.
5. Astræa,	1845, December 8,	Hencke.
6. Hebe,	1847, July 1,	Hencke.
7. Iris,	" August 13,	Hind.
8. Flora,	" October 18,	Hind.
9. Metis,	1848, April 25,	Graham.
10. Hygeia,	1849, April 12,	Gasparis.
11. Parthenope,	1850, May 13,	Gasparis.
12. Clio,	" September 13,	Hind.
13. Egeria,	" November 2,	Gasparis.
14. Irene,	1851, May 20,	Hind.
15. Eunomia,	" July 29,	Gasparis.

V. Miscellaneous Intelligence.

1. *Meteorological Instruments.*—The barometers and thermometers used under the direction of the Smithsonian Institution, and in the various academies of Massachusetts and New York, where they have recently been placed by Prof. Guyot, are made by James Green, No. 422 Broadway, New York. We copy the following from the recent Report of the Institution, (1851) p. 17.—The barometer has a glass cistern with an adjustable bottom enclosed in a brass cylinder. The barometer tube is also enclosed in a brass cylinder, which carries the vernier. The whole is suspended freely, from a ring at the top, so as to adjust itself to the vertical position. the bulb of the attached thermometer is enclosed in a brass envelop communicating with the interior of the brass tube, so as to be in the same condition with the mercury, and to indicate truly its temperature. Each instrument made according to this pattern, is numbered and accurately compared with a standard. In the comparisons made by Professor Guyot, a standard Fortin barometer, by Ernst of Paris, was employed; also a standard English barometer, by Newman, of London, belonging to the Institution. These instruments, for greater certainty, have been compared with the standard of the Cambridge Observatory, and of Columbia College, both by Newman; also with the standard of the Observatory of Toronto, Upper Canada.

The results of these examinations prove the barometers made by Mr. Green, according to the plan adopted by the Smithsonian Institution, to be trustworthy instruments.

The thermometers are by the same maker, and those intended for the State of New York, were compared with a standard by Bunsen of Paris, and with another by Troughton & Simms of London. Those found to differ more than a given quantity from the standards were rejected.

The instruments for detecting the variation of the hygrometrical condition of the atmosphere, consist of two thermometers, of the same dimensions, accurately graduated. The bulb of one of these is en-

veloped in a covering of muslin and moistened with water, and that of the other is naked.

The rain and snow gauges, and also the wind vanes, are made under the direction of the Institution, by Messrs. Pike & Son, 166 Broadway, New York. The rain gauge is an inverted cone of sheet zinc, of which the area of the base is exactly one hundred square inches. This cone or funnel terminates in a tube which carries the water into a receiving vessel. The water which has fallen is measured by pouring it from the gauge into a cylinder, so graduated as to indicate hundredths of inches. A smaller cylinder is also provided, which gives thousandths of inches, and may serve, in cases of accident, as a substitute for the larger cylinder. The rain gauge is placed in a cask sunk in the earth, with its mouth near the level of the ground.

The snow gauge is a cylinder of zinc of the same diameter as the mouth of the rain gauge. The measurement is made by pressing its mouth downwards to the bottom of the snow, where it has fallen on a level surface, then carefully inverting it, retaining the snow, by passing under it a thin plate of metal. The snow is afterwards melted, and the water produced is measured in one of the graduated glass cylinders of the rain gauge.

The wind vane is a thin sheet of metal, (it might be of wood,) about three feet long, carefully balanced by a ball of lead, and attached on the top of a long wooden rod, which descends along the wall of the building to the sill of the window of the observer. It terminates in the centre of a fixed dial plate, and indicates in its movements the direction of the wind by a pointer attached to the rod.

The observer is by this arrangement enabled to determine the course of the wind, by looking down on the dial plate, through the glass of the window, without exposing himself to the storm.

Besides the full sets of instruments furnished by the State of New York, from the appropriation of the Regents of the University, the Smithsonian Institution has furnished a number of sets, to important stations, and in order that they might be more widely disseminated, we have directed Mr. Green to dispose of sets, to individuals, at a reduced price, on condition that they will give us copies of the results of their observations; the remainder of their cost being paid by the Institution. A number of persons have availed themselves of this privilege.

To accompany the instruments, and for the use of those who take part in the Smithsonian system of meteorological observations, a series of minute directions, prepared by Professor Guyot, has been printed by the Institution. It occupies forty octavo pages, with wood-cut representations of the instruments, and is accompanied by two lithographic engravings, to illustrate the different forms of clouds, and to facilitate their notations in the journals, in accordance with the nomenclature adopted by meteorologists. A set of tables has also been furnished for correcting the barometrical observations, on account of variation of temperature. A set of hygrometrical tables, to be used with the wet and dry bulb thermometers, and a set, for the calculation of heights by the barometer, will be prepared.

2. *Notice of Meteoric Iron in the Mexican Province of Sonora;* by JOHN L. LE CONTE, M.D.—In February, 1851, while at Tucsan in

Sonora, I saw two large masses of iron, evidently meteoric, which were used as anvils by the two blacksmiths of that town. They were irregular in form, and although imbedded in the ground to make them steady enough for use, they were about three feet high. I endeavored to have some pieces cut off, and although a high price was offered, the characteristic Mexican indolence could not be overcome. The only answer I could obtain, was the metal was "muy duro."

These pieces were brought from a valley in a small mountain chain, about forty miles southeast of Tucsan, east of the road leading to Tuvaca. In this valley, fragments similar to those seen, and of various sizes were said to be abundant. From the occurrence of this metal, the valley was called "Cañada de Hierro," or iron valley. Silver mines of great richness are very numerous in that vicinity: the metal occurs as sulphuret, with galena and blende, and also in the native form.

3. *On the Improvements by Mr. Chas. A. Spencer in Microscopic Object-glasses*, (from a letter from Mr. CHAS. A. SPENCER, addressed to Prof. HORSFORD and Dr. BURNETT, published in the Boston Traveller.)—The following letter from Mr. Chas. A. Spencer, the celebrated maker of Microscopes at Canastota, N. Y., was received by Prof. Horsford and Dr. Burnett, as an answer to some definite inquiries made concerning the method by which his extraordinary results have been obtained. It was not intended for publication, but this use is made of it on account of its scientific importance. It contains a clear and concise statement of the difficulties and perplexities that attended those wonderful improvements in the manufacture of lenses, recently made by him; and, on account of which, it will be remembered that the Committee appointed at the Albany meeting of the American Association for the Advancement of Science, held in August last, decided that his glasses are quite superior to any now made in the world. On this account the letter will be read with interest by all, and especially by those who are versed in the optics of Microscopy. H. & B.

* * * "If I properly understand your request, it was that I should give you a statement of the peculiarities of construction, &c. which distinguish my microscopic objectives from those heretofore made.

To do this with sufficient clearness, it will be necessary for me to explain briefly the defects of the best objectives at the time I commenced my investigations. At that time the largest angle of aperture obtained by any optician, did not exceed 75° with even the highest powers. The objectives of Pritchard, Chevalier Oberhauser, Plœssel and Schieke, rarely reach this number of degrees, even at the present time. Owing to the character of the materials at the command of the working opticians, in connection with the fact, that all the existing works upon science were silent in reference to any formula or laws of combination, the construction of objectives had become nearly stationary as regarded their most essential character—*that of angle of aperture.*

The method then employed by the best artists was one entirely practical; and consisted merely in combining three double achromatics, (separately corrected for figure and color, or nearly so,) into one objective; and the differences which were to be found in the objectives of different artists, consisted mainly in the focal lengths employed to make up a given objective, most artists combining three objectives of

the same focal lengths and aperture. It is obvious that by such methods only fortuitous results could be obtained, no data existing by which new combinations could be calculated without vast labor, and the artist himself entertaining no hope that his materials were capable of yielding higher results.

The reason of this conclusion may be briefly stated. At an early stage of the improvements of the achromatic microscope, it was assumed as essential, that the different combinations should be so calculated as to permit the inner surfaces of the crown and flint lenses to be cemented, in order to prevent alike the loss of light and the indistinctness which would arise from so many otherwise reflecting surfaces. This practical rule being assumed, the construction of an achromatic object-glass, of the simplest form, became a determined problem, or nearly so; and the artist was confined within very narrow limits as to available radii and apertures with the best material he could command,—the crown, and dense flint glass of Guinaud.

The limit to the increase of aperture of an object-glass of given focal length, was soon found in the inequality of correction for sphericity; a marginal over-correction, and a central under-correction for figure, being the result of all attempts beyond this limit.

Such was the condition of optical knowledge applicable to the microscope, when the paper of Lister in the Phil. Trans. gave a new impulse to improvement, by making known some laws of combination before unnoticed, and calling attention to some characters of single achromatics, which might be made available in the formation of unusual combinations; which paper, up to the present time, is the only one that has appeared on this intricate subject. Improvements followed rapidly upon the appearance of this essay; and soon again reached a limit, beyond which the artist had no expectation of success.

So late as the year 1844, Mr. Ross, then, as now, the first of European artists, in announcing that, with his $\frac{1}{12}$th objective, he had reached the angle of aperature of 135°, declared, that a pencil of that angle, was 'the largest that could be passed through a microscope object-glass.'

It was under such circumstances of embarrassment, or rather of apparent hopelessness, that I continued the series of investigations, before began, which had for its object the improvement of the microscopic object-glass, in the very particular which was declared impossible by Mr. Ross.

Becoming satisfied at a very early period of my labors, that no real success could follow upon merely practical investigations; and feeling assured also that opticians had entirely neglected an essential element of their education,—that of an earnest study of the optical and physical characteristics of the materials upon which their skill was exercised, I undertook a series of experimental investigations in the manufacture of glass for optical purposes, with a view of supplying this deficiency in my knowledge, and of definitely solving the question of maximum angle of aperture of objectives.

Many new, unexpected, and valuable results were obtained, in reference to both the optical and physical characters of this interesting material. During the long course of labor and study, which these inves-

tigations rendered necessary, new laws of combination appeared, involving intricate theoretical investigations, and giving promise of results before unattained, not only with the new materials, but (from an increased knowledge of its optical characters,) with those also, in common use among artists.

To fulfill the conditions which were imposed by these new laws, in connection with those previously announced by Lister, it became a practical question how far skill in manipulation, to give the requisite curves, and mechanical construction to secure most, if not all, the practical conveniences required by the observer, could be made available.

Two and a half years since, I satisfied myself and others, that the limit assigned by the high authority of Mr. Ross, to the angle of aperture, was erroneous, and that this limit had no existence short of 180°. The question whether theory placed bounds to human skill, being thus satisfactorily determined, a question no less serious arose as to the means to be employed to make the largest attainable angle of aperture available, without sacrificing those conveniences which alone would render such an angle useful. The difficulty of this mixed theoretical and practical question will be understood from the fact that, more than two years since, angles of aperture of 170° were obtained by me, but the sacrifice of the working focus seemed then an insuperable bar to the employment of such objectives, as ordinary working and available powers. Every increase of their capacity in one direction, seemed made at the inevitable expense of some equally necessary capacity in another.

Nearly two years of unremitting thought have been given to the solution of this question alone. Six months since, a careful re-examination of the whole subject enabled me to surmount this last obstacle in the way of improvement,—leaving the field clear to those recondite investigations, which alone are wanting to place the seal upon the microscope as a perfect instrument.

Such, then, is a brief sketch of my labors in connection with this subject; and you will readily see, that to no one particular combination or discovery, has any success of mine been especially owing;—but to a lengthened study of the characters of the materials themselves,—the discovery of new qualities and capacities in glass, to new laws of combination, and to the union of these various acquisitions, so as to make practically available, those excellencies which were indicated by theory.

4. *On copying Copper Plate Engravings on Stone*, (from a letter from Lieut. E. B. Hunt, to J. D. Dana, dated New York, Jan. 17, 1852.)—I enclose some specimens of lithographic transfer printing of the plates to be in the forthcoming Coast Survey Report. I assume that you are interested in any thing of this kind, and the specimens we are now obtaining show so great an advance that it is worth reporting.

The process is briefly as follows. A copper plate being duly engraved, it is inked and an impression is taken on transfer paper. A good paper, which wetting does not expand, is needed, and a fatty coating is used in the process. The transfer paper impression is laid on the smooth stone and run through a press. It is then wetted, heated and stripped off from the stone leaving the ink and fat on its

face. The heated fat is softly brushed away leaving only the ink lines. From this reversed impression on the stone, the printing is performed just as in ordinary lithography. A good transfer prints from three to five thousand copies. Thus prints from a single copper plate can be indefinitely multiplied, the printing being moreover much cheaper than copper plate. The enclosed specimens which are only fair ones, show the applicability of this process to very fine topography and other fine engraving.*

5. *Note to Dr. Kirtland's paper, page* 215.—A lower degree of cold has recently been experienced throughout the west than occurred during the ten years preceding.

On the 16th and 17th of December, ult., the temperature was as follows, viz.:

	Dec. 16.	Dec. 17.
At Cleveland,	−6	−5
Painesville,	−8	—
Sandusky,	−8	—
Elyria,	−12	—
Ravenna,	−12	—
Akron,	−14	−17
Zanesville,	−9	—
Columbus,	−5	—
Marietta,	−3	−2
Cincinnati,	−4	−3

The schedule places the extreme of cold six degrees lower on the margin of the lake, than was stated in the foregoing article; yet the modifying influence of this body of water were equally apparent on that occasion. It will be observed that at Cincinnati and Marietta, lying at the extreme southern part of the state, with the advantage of high ranges of hills to screen them from the north, and a great amount of local heat generated from animal life and artificial fires, the mercury fell as low within two or three degrees, as at this point where nothing intervenes, except the lake, to arrest the winds from the north pole, or mitigate the cold.

At Painesville and Sandusky a little more remote from its influence, the mercury fell to 8° below zero; while in the interior of the state, it sank several degrees lower. During those two days the general cold seemed to contend for predominance with the warmth of the lake. Even at the low temperature which prevailed, warm emanations were constantly arising from the water and exhibited a beautiful phenomenon when viewed from the perpendicular bank of the lake, which rises 80 feet above its level, at this point. The warm vapors ascended several feet into the air, then condensed, congealed and fell back again in such rapid succession as to cut off from view the water, and to give to the unlimited expanse of the lake, the appearance of an immense cauldron, waiving or boiling like plaster of Paris parting with its water of crystallization at a high temperature.

* The specimens sent us fully sustain Lieut. Hunt's remark. His letter continues, "The establishments in New York which do good transfer work, are J. Ackerman's, 379 Broadway, and D. McLellan, Spruce street. Both are employed on the Coast Survey Report."

It was a cloud of these snowy spicula, thus formed, which enveloped the steamer Mayflower, and resulted in her running ashore. Passengers on board of her, though surrounded with falling snow, could occasionally discern the sun and clear sky overhead. The occurrence of this extreme cold at a time when the lake contained no ice, and the water was comparatively warm, was extraordinary. Our coldest weather usually happens in February, when the lake is extensively covered with ice. The northern birds seemed instinctively to foresee or anticipate the approach of a severe winter, for the white Arctic owl, pine grosbeak, red poll, and white snow birds appeared in the month of October; an occurrence never before observed.

6. *On the Cold of the month of January at Eutaw, Alabama*, Lat. 32° 46′ N. Long. 11° 3′ W. of Washington; *and on the Aurora of September* 29th, (from a letter from Mr. A. WINCHELL, dated Jan. 30.) —The cold of the present winter has been remarkable, and so far as I can learn, unprecedented. My thermometers for four days beginning with the 19th Jan., stood as follows:—

Day.	Sunrise.	9 A. M.	3 P. M.	9 P. M.	Mean.
19,	5	6	9	6	6·50
20,	2	8	21	19	12·50
21,	15	22	34	31	25·50
22,	18	20	28	21	21·75

I am unable to find any record of observations made in this latitude or lower, indicating an equal degree of cold. On the morning of the 18th December the thermometer stood at 9°. These are the extremes; but in general the winter has been characterized by almost uniform and severe cold.

While writing, I will add that the *aurora borealis* of the 29th September, was witnessed here as a very extraordinary and interesting phenomenon. I extract from a record made at the time: 'About 7 P. M., columns of blood-red light were seen streaming up in the northern and northeastern horizon. These were constantly changing their dimensions and forms, and new ones were continually appearing and disappearing. The longest streaks I estimated to extend to about 55° altitude. They were at this time all illuminated completely to the horizon and of a uniform blood-red color. At 8 P. M., there appeared in the north a well-defined arch of bright yellowish light. This was surmounted by normals tinged with evanescent hues of purple, green and red. The display lasted till near midnight.'

7. *Indian Hail-storms.*—Lieut. Col. SYKES, at the meeting of the British Association at Edinburgh (Rep. for 1850, p. 43) stated accounts of numerous hail-storms in India, obtained through the researches of Dr. Buist, LL.D., of Bombay. Dr. Buist says there is no account of the occurrence of hail within 1000 feet of the level of the sea south of latitude 20°, though just to the north of this hail-storms are very abundant, and they occur very frequently within the tropics at altitudes of 1700 feet and upwards. From the list, the hail-storms appear to have occurred 21 times in the month of April, 13 in March, 8 in February, 3 in January, 6 in May, 3 in June, 2 in September, 2 in November, and 2 in December. A few instances will suffice to show their character, and these are given from European testimony. On the 10th of

April, 1822, at Bangalore, a hail-storm killed many cattle, the hailstones being represented by the natives as large as pumpkins. *Three days after the storm* the gentleman who gives an account of it, says, "I went to the spot and found the carcases of twenty-seven bullocks lacerated by hailstones; also dead birds. In a tank 300 yards in circumference, half of the surface was covered with floating masses of hailstones which had been carried down the ravines two days before; some of the masses were five and a half inches in thickness; the hailstones were angular and oval, and some measured three inches in diameter."*

At Kamptee, on the 3d of June, 1823, an officer writes, "the hailstones without exaggeration were as large as pullets' eggs."

At Bopalpoor, on the 9th of February, 1825, an officer writes, "the hailstones were the largest and most extraordinary ever seen, some of them being as large and as heavy as goose-eggs, which they resembled."

At Serampoor in Bengal, on the 30th of March, 1827, the European writer says, "each of the hailstones was equal to the size of a goose's egg."

At Kotah, on the 5th of March, 1827, the hailstones were as large as a man's fist, and the next day remained unmelted of the size of pigeons' eggs. Men, animals and birds were killed; in the village of Nauda alone, six persons were killed and seven others dangerously bruised.

At Calcutta, on the 20th of April, 1829, the editor of the Bengal Chronicle says, "one of the hailstones brought to us was larger than a duck's egg;" many of them were angular fragments of ice, and several natives were killed.

At Serampore the hailstones were as large as hens' eggs, and consisted of coats like an onion; the nucleus was whiter than the exterior.

At Sylhet, on the 19th of February, 1830, the hailstones were of the size of the largest potatoes.† Sheep and goats were killed. At Jubbalpoor, on the 9th of April, 1831, the hailstones were of the size of guinea fowls' eggs. On the 10th of April, 1831, at Kamptee, some of the hailstones measured from ten to twelve inches in circumference; few or none were smaller than a hen's egg; five persons were killed in the neighborhood. At Alhabad, on the 5th of May, 1833, a hailstone weighed 6¾ ounces troy, and measured ten inches in circumference. At Chunar, on the same day, the gentleman writes, "blocks of ice fell; I am really speaking within bounds when I say a goose's egg was a trifle compared to some of the stones that fell; one measured 11½ inches in circumference." "I am informed," he adds, "one hailstone in the bazaar weighed *two pounds*." On the 16th of March, 1834, at Raneegunge, a gentleman traveling in a palkee, writes, "my palkee top yesterday was broke through in three places by hailstones, and one of my bearers knocked down by them." At Pubna, on the 12th of April, 1834, one of the hailstones measured a foot in circumference, and another weighed eleven ounces. At Benares, in February, 1836, some of the masses of ice weighed two pounds. At Secunderbad, on the 30th of March, 1837, some of the hailstones were two

* This was on the third day after the fall, in the scorching month of April.

† Potatoes in general are not much larger than hen's eggs in India.

inches in diameter. At Dum Dum, the artillery cantonment in Bengal, on the 8th of April, 1838, two hailstones were picked up which measured sixteen inches in circumference and more than five inches in diameter. At Jaulna, on the 14th of January, 1849, the hailstones were as large as billiard-balls. On the 5th of February, 1850, at Gwalior, pieces of ice fell nearly two pounds weight, and animals and some men were killed. At Condwiel, near Sattarah, on the 7th of April, 1850, some hailstones were as large as cocoa-nuts; the writer says, "I am within the mark when I say they were as large as cocoa-nuts."

8. *Meteorology of St. Bernard and Geneva.*—Prof. E. PLANTAMOUR, in the Bibliothèque Universelle, Nov., 1851, xviii, 177, has published an interesting paper on the climate of St. Bernard and Geneva, for the year 1850. The following table, made up from the several tables in the paper, present some of the results.

	Temperature.		Barometer.		Rain & Snow.		Intens. of winds.		Clearness of sky.			
	G. Cent.	St. B.	G. mm.	St. B. mm.	G. mm.	St. B. mm.	G. NE : SW	St. B. NE : SW	G. mean	St. B. mean.	G. days clear.	St. B. clear.
Jan.	− 2·52°	−11·00	726·18	558·46	41·5	146·6	1·05 : 1	2·20 : 1	0·89	0·55	1	9
Feb.	+ 4·00	− 5·58	731·20	565·35	23·5	131·8	0·50	25·29	0·53	0·42	8	14
Mar.	+ 2·40	− 8·22	728·93	562·34	4·7	38·5	2·14	7·36	0·83	0·33	16	17
Apr.	+ 8·09	− 3·70	723·07	559·88	128·8	133·2	1·25	2·30	0·76	0·78	1	0
May	+10·88	− 1·56	723·72	561·77	112·9	124·2	2·00	0·83	0·61	0·72	2	5
June	+16·76	+ 4·78	727·38	567·80	66·4	66·7	2·55	1·55	0·58	0·63	4	5
July	+17·68	+ 5·42	727·11	567·76	36·0	36·2	1·11	6·11	0·44	0·54	7	7
Aug.	+16·94	+ 5·03	727·62	568·12	85·7	108·7	0·70	1·46	0·51	0·68	8	3
Sept.	+12·66	+ 0·31	728·62	567·20	78·1	35·1	2·62	1·36	0·36	0·55	16	8
Oct.	+ 7·48	− 3·75	723·13	559·82	60·4	108·5	1·24	3·03	0·67	0·62	6	10
Nov.	+ 5·97	− 3·81	727·87	563·72	76·0	141·0	0·58	2·75	0·58	0·54	9	10
Dec.	− 1·38	− 6·03	730·51	564·37	23·7	48·9	1·34	3·04	0·87	0·33	1	19
Year	+ 8·49	− 2·26	727·11	563·86	737·7	1119·3	1·28	2·55	0·60	0·56	79	107

The mean temperature of Geneva for 1850 was about $\frac{1}{2}$ a degree less than for the ten years 1841–1850; and that of St. Bernard $\frac{4}{10}$ of a degree less. The greatest cold at Geneva was on the 11th of January, when it was — 13° 2 C.

The column of barometric observations for St. Bernard, give the mean pressure at noon. The mean pressure at 6 A. M. for the year is 0·35$^{mm.}$ less than at noon; at 6 P. M. 0·02$^{mm.}$ greater than at noon; at 8 P. M. 0·19$^{mm.}$ greater; at 9 P. M. 0·25$^{mm.}$ greater.

The rain and snow was of less amount than usual, and especially so at St. Bernard. The relative quantity for the seasons at Geneva was 12 p. c. in winter, 33 p. c. in spring, 26 p. c. in summer, 29 p. c. in autumn; and at St. Bernard, 29 p. c. in winter, 27 p. c. in spring, 18 p. c. in summer, 26 p. c. in autumn. Thunder was heard 22 days at Geneva, two times in each April and May, five times in each June, July and August, and three in September.

The northeast winds were unusually intense during 1850, especially at St. Bernard. In February, August and November, southwest winds prevailed at Geneva, and northeast at St. Bernard; and in May, the reverse was true. In the column of winds above, the ratio of N. E. to S. W. winds is given.

The sky was rather clearer than usual at Geneva in autumn, and somewhat less so in summer. At St. Bernard, the winter was clearer than usual, and the other seasons scarcely varied from the mean.

9. *Gold of Australia.*—We cite the following from a pamphlet on the Australian gold by Rev. W. B. Clarke, for which we are indebted to its author. The particular locality where gold was first discovered in 1851, was at the junction of Summer Hill creek with the Macquarie, 33 miles west of Bathurst and about 170 miles west of Sydney.*

"Years ago gold was found in small specimens by the earliest convicts; but the value of the indications was not then known, and the facts were concealed. In 1841, the author of these remarks again brought gold from the very basin of the river now supplying it: this gold was exhibited to members of government and of the legislature, in the Council Chamber itself, and to numbers of persons in the community, who have testified to the fact; it was spoken of openly; it has been discussed in public journals; it was made the ground of inquiries into the probable extensive auriferous character of the colony, and from the result of those inquiries, conducted on scientific principles, and backed by occasional additional proofs, it was openly declared, that gold exists in "considerable abundance" in our "schists and quartzites;" and further, the very region was pointed out in which it would be found. Still no one seemed willing to profit by the disclosure, made as it was cautiously, for reasons which can be appreciated when it is considered that the country was still a penal settlement. The fact was not doubted, but the public mind was directed to another channel. So, in England, though an illustrious geologist had declared his suspicion, that Australia offered in some respects a parallel to the Ural, and though another in Russia had done the same; and though the former had spoken, so late as 1849, to the Ministers of England, upon the subject of gold in Australia, all the encouragement he received was in being told not to let them '*have too much of a good thing.*'

It is not necessary now to detail the careful processes by which the author had convinced himself, that gold must exist in "cosiderable abundance," and that it would be found along the 149th meridian, to which he recommended, in 1850, the then expected geologist to be sent: nor is it necessary, to the vindication of scientific claims, to draw any comparison between his own convictions and the experience of others. The fact which he would insist on is, that it was left for one who makes no profession of geological science, but who had been in California, and by dint of observation, perseverance, and a series of fortunate accidents, was enabled to arouse attention, to excite the public to explore the very region, long known and previously proclaimed, as abundant in gold."

10. *Dead Sea.*—Humboldt states in his "Views of Nature," that M. Valenciennes had received beautiful specimens of *Porites elongata*, Lamk., from the Dead Sea. It is not mentioned whether this coral occurred where the water is supersaturated with salt, or only in the estuaries of rivers.

The following account given by Mr. Monk (son of the Venerable Bishop of Gloucester and Bristol,) of his feelings on first seeing and on taking leave of the shores of the Dead Sea, deserves special notice, not only because it appears true to nature, but because it suggests to

* The remark cited from Rev. Mr. Clarke, on p. 119, of this volume, should read "90° east of the Australian range" instead of "90° west of the Urals."

the mind the suspicion that other travellers, who have described the same scene, have been influenced by preconceived opinions, to see every thing in a gloomy point of view:—

"In about three hours, we reached the mountain brow looking down upon the valley of the Jordan; and delightfully that beautiful strange scenery burst upon our weary and dazzled eyes.

Far from looking gloomy or curse-stricken, it was the most *riant* scene I had yet beheld in Palestine. The Dead Lake itself was as brightly blue as those of Italy; the mountains of Moab and the Ammonites lifted their lofty line against the early sun, and wore a purple hue over their multiplied cliffs and promontories."

"Then came sunrise, first flushing the light clouds above, then flashing over the Arabian mountains, and pouring down into the rich valley of the Jordan. The Dead Sea itself seemed to come to life under that blessed spell, and shone like molten gold among its purpled hills.

I lingered long upon that mountain's brow, and thought that, so far from deserving all the dismal epithets that have been bestowed upon it, I had not seen so cheerful or attractive a scene in Palestine. That luxuriant valley was beautiful as one great pleasure ground, with bosks and groves of aromatic shrubs, intermingled with sloping glades and verdant valleys. The City of Palms might still be hidden under that forest, whence the old castle just shows its battlements. The plains of Gilgal might still be full of prosperous people, with cottages concealed under that abundant shade; and that dread Sea itself shines and sparkles as if its waters rolled in pure and refreshing waves 'o'er coral rocks and amber beds.'

The roads from hence to Jerusalem is drear and barren, and nothing but Bethany occurred to divert my thoughts from dwelling on the beautiful Dead Sea."—*From an article on the Dead Sea, by Dr. R. J. Graves, in Jameson's Edinb. J.*, li, 315.

11. *On the rapidity of the fall of Rain;* by M. Rozet, (Comptes Rendus, 1851, Nov. 24, 581.)—During a calm, the rain falls vertically, and, in such a case, a rail-car in rapid motion, affords a means of measuring the rapidity of the fall of the rain drops. The window of a car is a rectangle, the sides of which are vertical and therefore parallel, when at rest, to the lines of the falling drops. But immediately on giving the car motion, the drops appear to be inclined in a direction the reverse of the motion of the car, though still parallel to one another. One of the drops on its descent passes, say, by the upper angle of the window, and continues on so as to pass the vertical side, at a certain point in its height. If a line be drawn through this point parallel to the top of the window, we have a rectangle the horizontal side of which is to the vertical, as the rate of motion in the cars to that of the falling drop.

M. Rozet, when on a journey between Beaune and Dijon, encountered a shower of rain of large drops, the rate of the cars was 40,000 meters an hour; and this gave 11 meters a second for the rapidity of the fall of the rain.

12. *Means of preserving indefinitely Monuments consisting of Marble or Limestone;* by M. Rochas, (Comptes Rendus, Dec. 1, 1852, 622.)—M. Rochas observes that the calcareous marbles least liable to change or wear from atmospheric causes, are those containing silica in a state of

combination, and not as a mere mechanical mixture. Following out this idea, he has succeeded in subjecting limestone to a process of "*silicatisation*," by which it is partly changed to a silicate of lime. He suggests that valuable monuments and ancient relics of marble, undergoing disintegration and sometimes so soft that they can hardly be handled after exhumation, may in this way be rendered firm and enduring.

13. *Annual Mortality of various Cities of the United States, and other places.*—We extract the following from a valuable paper on the sanitary condition of New Orleans, by J. C. SIMONDS, M.D., from the Charleston Medical Journal and Review, Sept., 1851.—I here present the mortality of the cities of the United States, carefully calculated by myself from authentic data. The data, the principles of the calculation, and the authorities, will be hereafter published, the result only being here given.

Average Annual Mortality of various Cities of the United States.

Boston,	39 years,	1811 to 1849,		2·4572
Lowell,	13 "	1836 to 1848,		2·1194
New York,	45 "	1805 to 1849,		2·9622
Philadelphia,	34 "	1807 to 1840,		2·5510
Baltimore,	14 "	1836 to 1849,		2·4917
Charleston,	27 "	1822 to 1848,	Whites,	2·4826
			Blacks,	2·6458
			Both,	2·5793
Savannah,	8 "	1840 to 1847,	Whites,	4·1616
New Orleans,	4¼ "	1846 to 1850,		8·1017

Annual average Mortality of other places.

Massachusetts,*	1847–'48,	1·59
Twelve counties of England,†		1·93
Twenty-six cities†	"	2·72
London,‡	Males,	2·74
	Females,	2·31
Liverpool,‡	Males,	3·53
	Females,	3·15
Liverpool,§	1850,	2·73
Manchester,§	Males,	3·65
	Females,	3·31

14. *Eruption of Mauna Loa*, (From the Polynesian of Aug. 23d, 1851; communicated to the editors by D. D. BALDWIN.)—A Hilo correspondent under date of August 12, writes, "The great crater on Mauna Loa that was generally thought to be quite extinct, is now in action. For a few days a heavy cloud, having the appearance of smoke, has been observed to hover over the summit of the mountain. Last night the mountain stood out in bold relief, unobstructed by clouds or mist, and presented a sublime and awfully grand appearance, belching forth flames and cinders that fell again in showers at a distance."

A subsequent communication reports the eruption as having continued twelve days.

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* Calculated from Registration Reports. † Dr. Jarvis on Vital Force.—Appendix.
‡ M'Culloch's British Empire. § Chambers' Edinburgh Journal, March 1, 1851.

15. *Electro-Telegraphic Progress*, (Athen., No. 1261; from the Builder.)—The Submarine Telegraph Company are getting made several new metallic cables, in addition to that already in operation—one conductor being already insufficient to convey the multitude of despatches now exchanged between London and the continent. The facility and certainty with which the telegraph has worked, have already effected a great revolution in commercial arrangements, which would be thrown into confusion by the rupture of the communication. Night and day it is carried on. There is still a space of about a mile (from East Cliff to the Southeastern Telegraph Office) unconnected by the wires. The distance has to be done by horse express, and, consequently, causes a few minutes break in the communication. The desideratum is, however, to be speedily supplied. The number of telegraphic stations now open and in connection with the central station of the Electric Telegraph Company in Lothbury, amounts to 226, embracing all the principal towns in the kingdom. Nearly seventy are principal commercial stations, at which the attendance is day and night: the length of the lines of communication extend over 2,500 miles, with 800 in progress of suspension. Since the partial reduction of charges, it is said, persons of all classes are availing themselves of its advantages for business purposes.

16. *Gutta Percha in Photography*, (Athenæum, No. 1261.)—At the meeting of the Photographic Club on Saturday last, Mr. Fry exhibited some charming pictures on glass, obtained by a combination of gutta percha and collodion. To the ordinary collodion—gun-cotton dissolved in ether—a small quantity of gutta percha is added, which readily dissolves. This is employed with the ordinary materials for the processes on glass,—the picture being developed by pyro-gallic acid. The extraordinary sensibility of this preparation may be inferred from the fact, that a positive copy from a glass negative has been obtained in five seconds by gas-light. The film formed on glass is far more adherent than the ordinary collodion or albumen:—we may, therefore, expect many valuable results from Mr. Fry's discovery.

17. *Perturbations of Uranus*, (Athenæum, No. 1261.)—The Paris correspondent of the *Literary Gazette* says:—"A curious fact for astronomers has just been ascertained. In the papers of the celebrated Lalande, recently presented to the Academy of Sciences by M. Arago, there is a note to the effect that so far back as the 25th of October, 1800, he and Burckhardt were of opinion, from calculations, that there must be a planet beyond Uranus, and they occupied themselves for sometime in trying to discover its precise position."

18. *On the Structure of the Lunar Surface, and its relations to that of the Earth;* by James Nasmyth, Esq., (Jameson's Edinburgh New Phil. Journal, li, 267, 1851.)—Some of the principles brought forward in this paper by Mr. Nasmyth, will be found in a paper by J. D. Dana in this Journal, 2nd series, ii, 335–355, 1846, and iii, 94, 1847.

19. *Apteryx.*—A live apteryx has reached the zoological gardens, London.

OBITUARY.

James E. Dekay.—Dr. Dekay died on the 21st of November last, at his residence on Long Island, at the age of 59. He is extensively

known for his various researches in natural science. He was educated for the medical profession, and pursued his studies in Edinburgh, Paris and Germany. His travels at this time extended into Turkey, and in 1831, 1832, he published his *Sketches of Turkey*. After his return, his attention was soon given to the study of natural history, and the principal results of his labors are found in many articles published in the Annals of the Lyceum of Natural History of New York, and in his voluminous reports in 4to, on the Zoology of the State of New York. In the social relations of life, his uprightness, amiability and cheerful temperament, endeared him to all who shared in his acquaintance. The vast labors, demanded of him in the preparation of his State Reports on Zoology, impared his health, which he never afterward fully regained.

Prof. EDWARD LASSELL.—Prof. Lassell, till recently Professor of Chemistry in Williams College, Mass., died at Auburndale, near Boston, on the 31st of January last, aged 40.

VI. BIBLIOGRAPHY.

1. *Correspondence in relation to a universal system of Meteorological Observations for the Sea as well as for the Land.* 30 pp., 12mo.—A proposition to the United States government from Great Britain, to coöperate in a system of meteorological observations at foreign stations, commences the correspondence in this pamphlet. Lieut. Maury, U.S.N., who is laboring so assiduously in this department, and to whom the above proposition was officially communicated, discusses the importance of such a system of combined effort, setting forth the steps necessary to ensure success, and the benefits to accrue to science and the world. He suggests that the plan should include France, Germany and Russia as well as Great Britain; also, that it should embrace the sea as well as the land, and that the merchant and naval service equally with foreign consuls should be enlisted. For this purpose, a meteorological conference is proposed, and communications have been addressed to the diplomatic functionaries of the various governments in Washington, requesting them to bring the subject to the notice of their governments. Great results would undoubtedly follow from such a conference: and we should hope that it might end, not only in a uniform plan of operations, but also in a uniform scale for the different instruments. An incalculable amount of computation for the comparison of observations would thus be saved.

2. *Fifth Annual Report of the Board of Regents of the Smithsonian Institution.* 326 pp., 8vo. Senate Doc., Special Session, March, 1851. —The Smithsonian Institution is doing a noble service for the country in various ways. Systematic scientific researches and explorations are encouraged by it, plans and directions as to modes of investigation are published and distributed, and efforts are made to give uniformity to the instruments and other means of observations.* Prof. S. F. Baird, the Assistant Secretary, a thorough and accomplished naturalist, has special charge of natural science. Scientific memoirs of high value, often with expensive illustrations, are brought out and placed within the reach of the public, thus giving an impetus to research and at the

* See our citation, page 288.

same time distributing its results among those who will render them available to the country. A library of indefinite extent is in the course of collection, and the labors of the able librarian, Mr. Jewett, are devoted also to the general progress of libraries throughout the country. By these and other modes, the will of Smithson is faithfully carried out by the Institution under the direction of Prof. Henry, its learned Secretary. The Report gives details of its various operations of a most gratifying kind.

3. *On the connection of Geology with Terrestrial Magnetism*, showing the general polarity of matter, the meridional structure of the crystalline rocks, their transitions, movements and dislocations, including the sedimentary rocks, the laws regulating the distribution of metalliferous formations and other terrestrial phenomena; by EVAN HOPKINS. 2nd edition, 200 pp., 8vo, with 30 plates and numerous wood-cuts. London, 1851.—The above title gives a general view of the contents of Mr. Hopkins's works. The prime idea at the basis of his speculations and conclusions, is found in magnetism.

4. *Photography:* a Treatise on the Chemical changes produced by Solar Radiation and the production of Pictures from Nature by the Daguerreotype, Calotype and other Photographic processes; by ROBERT HUNT, Prof. of Mech. Science in the Museum of Practical Geology, author of Researches on Light, the Poetry of Science, etc.; with additions by the American editor. 266 pp., 12mo. New York, 1852. S. D. Humphrey.—Prof. Hunt, the author of this work on photography is well known for his able investigations in this department of the arts, and his profound knowledge of many branches of physical science. His work is the result of experience as well as study, and should be in the hands of all engaged in photographic operations, both the professed artist and the amateur. The author treats of the history of the science, its theory, and its various processes on paper, silvered plates and other materials, giving all the minuteness of detail required by a learner in the subject, including everything published up to the date of publication, 1851. Among the additions by the American editor is a chapter on the American daguerreotype process. The book is well illustrated by wood-cuts representing instruments, besides containing portraits of Daguerre and Neipce, and two lithographs representing a positive and negative photograph.

5. *The Indications of the Creator, or the Natural Evidences of Final Cause;* by GEORGE TAYLOR. 282 pp. 12mo. New York, 1851. C. Scribner.—The object of this volume is to sustain the idea of a personal Creator, against certain pseudo-scientific arguments, and at the same time to present a review of the more interesting discoveries in science. The development theory occupies a large part of the volume,—perhaps not too large, considering the extent to which the views of the Vestiges of Creation have been circulated. Yet a reader might gather the idea that science was largely falling into this line of error, instead of the actual fact, that no scientific man in Britain or America, had given his name in support of the theory in any publication, and more than this, that science has stood so firmly against it, and especially geologists, that the author of the Vestiges, in his supplement, actually endeavors to prove that men of science, are men of one idea and therefore not competent to judge.

6. *Occultations visible in the United States during the year* 1852; computed by JOHN DOWNES, at the expense of the fund appropriated by Congress for the establishment of a Nautical Almanac, published by the Smithsonian Institution. 34 pp., 4to. Washington, 1851.

7. *Catalogue of Malayan Fishes.*—The Journal of the Asiatic Society of Bengal for October and December, 1849, contains an elaborate catalogue of the Malayan fishes, with annotations and descriptions, by THEODORE CANTOR, Esq., M.D., of the Bengal medical service. It occupies 460 pages.

8. *Ray Society: British Nudibranchiate Mollusca, with figures of all the species;* by J. ALDER and A. HANCOCK, Part V, 4to. London, 1851.—This volume is the second and remaining volume, of the Ray Society publications, for the year 1850. It contains sixteen 4to lithographic plates, partly colored, illustrating numerous species, and giving details of structure. Part VI will finish this valuable work.

Ray Society: The British species of Angiocarpous Lichens, elucidated by their Sporidia; by the Rev. W. A. LEIGHTON, B.A., &c. London, 1851. 102 pp., 8vo with 30 plates.—This work appears as the first volume for 1851; the second will be a monograph on the Cirripeds by Mr. Charles Darwin.

The Ray Society is contributing much to the progress of science and especially British science. The works published are of high value and authority, and are copious in their illustrations of species. Subscriptions (the sum is a guinea a year) are solicited from all interested in science.

9. *An Introduction to the Atomic Theory;* by CHARLES DAUBENY, M.D., F.R.S., &c. 2nd edition greatly enlarged. 502 pp., 16mo. Oxford, 1850.

10. *Annals of the Lyceum of Natural History of New York.* Vol. V, No. 3, pp. 78–120, and Nos. 4, 5, pp. 121–184.

C. B. ADAMS: Descriptions of new species and varieties of the land shells of Jamaica, p. 77.—New freshwater shells of Jamaica, p. 98.—On the habitats of certain species of land shells, p. 100.—Catalogue of the land shells of Jamaica, p. 103.

GEO. N. LAWRENCE: Descriptions of new species of birds, with a plate, pp. 102 and 121.—Additions to North American ornithology, pp. 117 and 123.

JOHN L. LE CONTE: Descriptions of new species of Coleoptera, from California, pp. 125–184, and to be continued.

11. *Journal of the Academy of Natural Sciences of Philadelphia,* 2nd ser., Vol. II. Part II. pp. 81–184, 4to, with plates 10–16. Philadelphia, 1852.

Art. 9. R. C. TAYLOR: Geological notes on the auriferous porphyry region next the Caribbean Sea, in the province of Veraguas and Isthmus of Panama, p. 81, pl. 10.

10. S. W. WOODHOUSE: The North American Jackal, Canis frustror, p. 87.

11. D. D. OWEN and B. F. SHUMARD: New Crinoidea from the sub-carboniferous limestone of Iowa and Illinois, p. 89, pl. 11.

12. J. Cassin: Descriptions of Owls presumed to be new, in the collection of the Academy. p. 95, pl. 12.

13 and 18. J. L. Le Conte: An attempt to classify the Longicorn Coleoptera of the part of America north of Mexico, pp. 99 and 139.

14. J. Cassin: Monograph of the Birds composing the genera Hydropsalis, *Wagler*, and Antrostomus, *Nuttal*, p. 113, pl. 13 Antrostomus, 14 Hydropsalis, (referred to as pl. 12, 13 in the text.)

15. I. Lea: On the genus Acostæa of D'Orbigny, p. 125.

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17. J. Leidy: Description of a new species of Crocodile from the Miocene of Virginia, p. 135, pl. 16.

19. D. D. Owen: Descriptions of two new minerals and a new earth, p. 179.

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UofM Mou